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21 to December 13 1900

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PROCEEDINGS

OF

THE ROYAL SOCIETY.

June 21 19[illegible]

The LORD LISTER F R C S D C L President in the Chair

Mr George James Burch Dr Leonard Hill Mr Joseph Jackson Lister Professor Arthur A Rambaut Mr William James Sell Mr Philip Watts, and Mr Charles T R Wilson were admitted into the Society

The Right Hon Sir Ford North a member of Her Majesty's Most Honourable Privy Council, was balloted for and elected a Fellow of the Society

A List of the Presents received was laid on the table, and thanks ordered for them

The following Papers were read —

I ' On the Effect of Changes of Temperature on the Elasticities and Internal Viscosity of Metal Wires By Professor A GRAY, F R.S , V J BLYTH, and J S DUNLOP

II ' On the Connection between the Electrical Properties and the Chemical Composition of Different Kinds of Glass Part II By Professor A GRAY F R S and Professor J J DOBBIE

III " On the Change of Resistance in Iron produced by Magnetisation By Professor A GRAY, F R S and Professor E T JONES

IV " Underground Temperature at Oxford in the Year 1899, as determined by Five Platinum Resistance Thermometers " By Dr A A RAMBAUT Communicated by E H GRIFFITHS, F R.S

V " On the Kinetic Accumulation of Stress, illustrated by the Theory of Impulsive Torsion." By Professor K. PEARSON, F R.S

VI. "Lines of Induction in a Magnetic Field." By Professor HELE-SHAW, F.R.S., and A. HAY.

VII. "On the Spectroscopic Examination of Colour produced by Simultaneous Contrast." By G. J. BURCH, M.A. Communicated by Professor GOTCH, F.R.S.

VIII. "An Experimental Investigation into the Flow of Marble." By Dr. F. D. ADAMS and Dr. J. T. NICOLSON. Communicated by Professor CALLENDAR, F.R.S.

IX. "A Criticism of the Young-Helmholtz Theory of Colour Perception." By Dr. F. W. EDRIDGE-GREEN. Communicated by Sir LAUDER BRUNTON, F.R.S.

X. "On the Viscosity of Gases as affected by Temperature." By LORD RAYLEIGH, F.R.S.

XI. "On the Thermodynamical Properties of Gases and Vapours as deduced from a Modified Form of the Joule-Thomson Equation, with Special Reference to the Properties of Steam." By Professor H. L. CALLENDAR, F.R.S

XII "Note on Inquiries as to the Escape of Gases from Atmospheres." By Dr. G. JOHNSTONE STONEY, F.R.S.

XIII. "The Distribution of Molecular Energy." By J. H. JEANS, B.A. Communicated by Professor J. J. THOMSON, F.R.S.

XIV. "Energy of Rontgen and Becquerel Rays, and the Energy required to produce an Ion in Gases." By Professor E. RUTHERFORD and R. K. McCLUNG. Communicated by Professor J. J. THOMSON, F.R.S.

XV. "On the Capacity for Heat of Water between the Freezing and Boiling Points, together with a Determination of the Mechanical Equivalent of Heat in Terms of the International Electrical Units." By Dr. H. T. BARNES. Communicated by Professor CALLENDAR, F.R.S.

XVI. "On Expressed Yeast-cell Plasma (Buchner's *Zymase*)." By Dr. A. MACFADYEN, Dr. G. H. MORRIS, and S. ROWLAND. Communicated by Sir H. E. ROSCOE, F.R.S.

The Society adjourned over the Long Vacation to Thursday, November 15.

Investigations on Platinum Thermometry at Kew Observatory

By C CHREE ScD LLD FRS Superintendent

Communicated by the KEW OBSERVATORY COMMITTEE Received December 5 —Read December 14, 1899

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Preliminary

§ 1 In 1895 the Kew Observatory Committee decided in the words of their annual Report to install platinum thermometers at Kew and to institute an independent series of experiments into their behaviour The Report adds, "Attention will in the first instance be directed more especially to the question of the fixity of the zero and of the fundamental interval

In pursuance of this policy the Committee built a special room, furnished with a fume-closet, and purchased from the Cambridge Scientific Instrument Company six platinum thermometers and a Callendar Griffiths resistance bridge, which was regarded at the time as embodying all the latest improvements

In the choice of apparatus and the construction of the room, the Committee had the advantage of the advice of Mr E H Griffiths, while Mr C T Heycock and Mr F H Neville, as well as Mr Griffiths, kindly visited the Observatory, and illustrated the methods of using the apparatus.

An account of the original installation was given by Mr. Griffiths in 'Nature,' Nov. 14, 1895, pp. 39–46. As this paper and several more recent papers by Professor Callendar and others have discussed the fundamental facts of platinum thermometry very fully, I have judged it unnecessary to go into such details here.

Thermometers.

§ 2. The six original thermometers, distinguished as K_1 to K_6, were all made from one sample of platinum wire. In 1896 a seventh thermometer, K_7, was obtained from the Instrument Company. It is believed to be of the same sample of platinum wire as the others, but this is not absolutely certain. Particulars as to the type of the thermometers are given in the following table:—

Table I.

Thermometer.	Material of tube.	Length from end to edge of wooden collar.	Length from end to terminals.	Diameter of tube.	Approximate resistance in ohms, answering to fundamental interval.
		cm	cm	mm.	
K_1	porcelain	33·0	37·5	11·5	1·0
K_2	"	35·5	40·0	11·5	1·0
K_3	"	26·5	31·0	13·5	1·0
K_4	"	26·5	31·0	13·5	1·0
K_5	glass	35·5	41·5	10·5	1·0
K_6	"	35·0	41·0	14·0	2·5
K_7	"	34·5	41·0	8·0	1·0

The resistance at 0° C. of K_6 is about 6·5 ohms, while the resistance of each of the other thermometers at 0° C. is about 2·6 ohms.

Since it came to Kew, K_6 has been exposed to no temperature above the steam point; whilst K_3, K_4, K_5, and K_7 have not been taken above the sulphur point (444°·53 C. according to Callendar and Griffiths, under normal pressure). In their early days, K_1 and K_2 were heated on several occasions to the temperature of melting silver (approx. 960° C.).

After a few months' use, K_1 began to behave unsatisfactorily. The Instrument Company reported after inspection that the tube was slightly short, and a new tube was fitted in March, 1896. The career of K_2 has been chequered. The first tube broke in melting silver in

March, 1896; the second tube was found to be broken in August, 1896, after a silver point experiment. After repair the thermometer behaved badly, and had to be sent to Cambridge. A third tube cracked in molten silver in December, 1896. The fourth tube lasted until the end of 1898, when it was broken at Sèvres. On more than one occasion the thermometer had practically to be remade, so that the observations taken with K_2 at different stages of its existence are not comparable. The spiral at present in K_2 is believed, however, to be of the same sample of platinum as the original one.

The other five thermometers are not known to have had any misadventures.

The thermometers K_2 and K_5 were taken by Dr. Harker to Sèvres in July, 1897, and did not return to Kew until the end of 1898. No use was made of K_5 at Sèvres, but some observations were made with K_2 up to a temperature of about 600° C.

§ 3. In an ordinary platinum thermometer, it is possible for the air inside the tube to become unduly moist, with consequent deterioration of the insulation. In a glass tube the presence of moisture may be detected by the cloudy appearance when the thermometer is cooled in ice; in the case of a porcelain tube the only guide is the behaviour of the galvanometer. If there is no sensible creep in the galvanometer, it is probably best to leave the thermometer alone, even if slight cloudiness is visible. There is some risk of altering the apparent zero of the thermometer in removing it from the tube, and replacing it after the tube has been dried out. On the rare occasions when a tube has been dried out, check observations have been taken before and after the process. The room in which the platinum thermometers are kept is heated night and day when necessary by a gas stove, whose combustion products escape by a flue opening outside. As the room is naturally a dry one, the risk of moisture has thus been small.

Resistance Box.

§ 4. The box as it originally existed in 1895 had a plug system similar to that of the ordinary Post Office pattern. The plug holders were of brass. Towards the end of 1896 it was found that pulling out a plug influenced the tightness of its neighbours. Early in 1897 the Committee arranged with the authorities of the International Bureau of Weights and Measures that Dr. J. A. Harker should proceed to Sèvres, and take part in a comparison of platinum and gas thermometers. At first it was proposed to take the existing Kew box to Sèvres; but, on hearing of the difficulties experienced with it, the Committee decided that Dr. Harker should examine into their reality before a decision was come to. Finding that with an ordinary standard of plug tightness sufficiently consistent results were not attainable, Dr. Harker tried the effect of greater tightness This, however, made matters rapidly worse, and it shortly became obvious that the box in its existing condition was of no further use; it was accordingly sent for repair to Cambridge.

Having high hopes of a new system of plug holders—fusible metal inside Doulton ware—the Instrument Company introduced this, at their own expense. The plug holders in the restored box are supported separately, and the pulling out of a plug has never shown any tendency to influence others. This is undoubtedly a great improvement; but I am somewhat doubtful of the expediency of the other changes made when restoring the box.

After some time it was found that taking out and replacing a plug sometimes exerted a very sensible influence on the reading, and from the appearance of the plugs it was suspected that this arose partly from the state of the plug holes The fusible metal seems disposed to develop a coating of light-coloured powder, and presumably this affects the plug resistances. On being applied to, the Instrument Company supplied a simple arrangement for cleaning out the plug holes without undue friction, and it has certainly improved matters.

The accompanying sketch, fig. 1, shows diagrammatically a vertical section of the original box, perpendicular to its longest dimension. C represents the coil chamber, AAA a copper tank containing water. This tank could be heated from below by a gas burner, the flow of gas being determined by a gas regulator, whose mercury bulb was surrounded by the water inside A. The coils hung in air, and their temperature was deduced by means of a mercury thermometer whose bulb was inside C.

When altering the plug system, the Instrument Company altered the shape of the water tank and the form of the coil chamber. Fig. 2 gives a section of the existing form taken in the same way as fig 1,

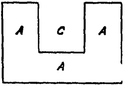

FIG. 1.

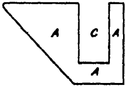

FIG. 2.

the letters having the same significance. The changes were presumably dictated by the altered nature of the plug arrangements. I am disposed to think that the coil chamber in the existing form is not so well protected from external influences as in the earlier form, and I believe that in accepting the readings of the mercury thermometer, as giving the temperature of the coils, there is more risk of error now than formerly.

Resistance Coils.

§ 5. The coil resistances are on the binary scale,

$$H = G/2 = F/4 = E/8 = D/16 = C/32 = B/64 = A/128 = Aa/256,$$

with two extra coils, "Cal" and FI—or I, as it will be called here—the former used merely in calibrating the bridge wire. The nominal value of H is 5 box units. The unit is very approximately 0·01 of an ohm, so that the fundamental intervals of all the thermometers except K_6 are nearly 100 box units This has the advantage that when the reading R_0 in ice is subtracted from the observed reading, one has approximately the temperature in degrees Centigrade on the platinum scale.

The coil I is approximately 100 box units, and practically all the fundamental interval determinations, except with K_6, have been referred to it.

The coil A*a* was added along with the coil Cal in 1897, when the box was altered. It has not been used in any of the work now to be described, and the mean box unit has been based throughout on the eight coils H to A. The coils are of platinum silver.

The bridge wire is fully 30 cm. long, readings to the right of the centre representing a temperature above, and readings to the left a temperature below that answering to the sum of the coils whose plugs are out. The bridge wire is also of platinum silver, and possesses the same temperature coefficient as the coils.

The bridge-wire scale is divided to 1 mm, and the vernier reads directly to 0·02 mm., and allows 0·01 mm. to be estimated. A difference of 1 cm. on the bridge-wire reading answers very nearly to 1 box unit, or to a difference of 1° in temperature with all the thermometers except K_6. Thus, in referring to differences of bridge-wire readings I shall usually, for brevity, speak of them as differences of temperature.

Nature of the Observations.

§ 6. The expediency of using the same coil combinations for all the thermometers except K_6 was soon recognised. Also a point has been made of using two combinations for each temperature, according to the following scheme:—

Thermometers.	In ice.	In steam.	In sulphur vapour.
K_1, K_3, K_3, K_4, K_5, K_7,	CDF CDGH	CDFI CDGHI	AE AFGH
K_6	AG AH	ACDF ACDGH	

By using the same coil combinations for different thermometers we may at least hope to detect any sensible *relative* changes, whilst by using two coil combinations we get a better idea of the order of accuracy of the observations, and have some check on the constancy of the smaller coil resistances.

As a rule, each fixed point determination depended on at least three observations. Thus, in ice we might have a balance against CDF, next against CDGH, and finally against CDF again. By comparing the readings in the first and third observations one can make reasonably sure that the thermometer has reached the true temperature. This is a very necessary precaution, especially in sulphur point observations.

When only three observations were taken, only half weight was allowed to the first and last, so as to give equal weight to the two combinations of coils.

It was originally intended to take a complete set of "fixed point" observations, ice, steam, and sulphur points, with each thermometer once a month. The regular observations have been in reality less frequent than was intended, and the intervals between them more irregular. In addition to the regular observations there have been a variety of special occasions on which ice and steam point determinations have been made, more especially with K_7. This thermometer has been used in comparing occasional high-range mercury thermometers.

Sources of Change or Error.

§ 7 Before considering the main question, I propose discussing a number of possible sources of change in the readings obtained with platinum thermometers. An error of invariable amount from a source external to the thermometer is perhaps not immediately germane to this inquiry, but the invariability of error is a very difficult thing to ensure.

Excluding observational error or defects, such as moisture in the tube, which an experienced observer is likely to detect, I would chronicle the following sources of trouble:—

1. Change in the resistance of the platinum spiral;
2. Change in the thick platinum wires connecting the spiral to the thermometer terminals, or in the compensating loop inside the tube;
3. Change in the relative resistance of the leads, or in the relative resistance of the proportional arms of the box;
4. Change in the box coils or in the bridge wire;
5. Faulty action of the contact piece;
6. Shift of the bridge centre;
7. Thermo-electric currents;
8. Heating due to the battery current;

9 Error in the temperature coefficient of the coils, or differences between the temperature coefficients of different coils,

10 Change of zero or other error in the mercury thermometer inside the coil chamber,

11 Failure of the box mercury thermometer to give the true temperature of the coils and bridge wire,

12 Insufficient immersion of the thermometer

13 Slowness of platinum thermometers in acquiring the true temperature

There are other possible sources of error affecting only certain "fixed point" observations, and mostly not peculiar to platinum thermometry, for instance —

14 Impurity in the ice used in getting R_0, or variability in the method of treating it,

15 Error in the barometer, whether constant or varying, or error in the reduction of its readings

16 Impurity in the sulphur used, and other uncertainties in the determinations of the boiling point of sulphur

17 Error in the formula assumed for the variation of the boiling point of sulphur with pressure

I have seen no adequate consideration of these sources of error in any treatment of platinum thermometry, and in the original equipment obtained for Kew Observatory no provision was made for the detection of most of them thus our experience may be useful to others Without at least a general idea of their nature, the reader would be unable to judge correctly of the degree of probability attending the conclusions reached, or even of the nature of the evidence on which these conclusions are based

§ 8 I propose deferring until the end the discussion of the evidence bearing on the first two items, only calling attention in the meantime to the fact that without taking a platinum thermometer to pieces it would be very difficult, if not impossible to distinguish between changes in the resistance of the spiral and changes in the resistance of the thick platinum wires connecting the spiral to the terminals The unalterability of a platinum thermometer cannot be proved by experiments on an isolated platinum spiral The connecting platinum wires inside the tube are by no means of negligible resistance They ought to be very approximately equal in resistance to the compensator loops intended to neutralise their variation with temperature The resistance of the compensator loops has been measured by Dr Harker with the following approximate results The data answer to a temperature of about 15° C

Table II.—Resistance of Compensator Loops.

Thermometer ……	K_1	K_2	K_3	K_4	K_5	K_6	K_7
Resistance (in ohms)	0·20	0·22	0·15	0·15	0·21	0·20	0·22

We may thus infer that the average resistance of the connecting wires is some 7 per cent. of that of the spiral in one of the ordinary thermometers. Thus a small differential change between these wires and the compensator loop might appreciably affect the thermometer readings.

The Cambridge Instrument Company inform me that the connecting wires and the compensator loop—which are of the same thickness of wire—are of the same platinum as the spiral and have the same temperature coefficient. If this were not the case a small difference between the absolute resistances of the connecting wires and the compensator would introduce a source of error fluctuating with the temperature of the wires and compensator.

It would, I think, be desirable to make sure that the process of attaching the thick wires to the fine spiral causes no change in the material close to the junction.

Changes in the Leads or in the Proportional Arms.

§ 9 In the leads supplied to Kew Observatory one pair has the terminals at both ends marked P, the other pair has its terminals marked C. This is intended to show that the former pair should be attached to the terminals marked P on the thermometer head and to the plugs P in the box, and that the other pair should be attached to the C terminals on the thermometer head and the plugs marked C in the box. Supposing this arrangement invariably adhered to, any *constant* difference between the resistances of the leads should not appreciably influence the apparent resistance of a platinum thermometer. Supposing, however, that a difference between the resistances of the leads develops itself, then if the leads are used in the way indicated above, the effect on the readings is the same as if the platinum thermometer itself changed.

Loosening and tightening the leads had been found early in the investigations to be a possible cause of slight variations in the reading —owing presumably to the varying tightness—and it may allow the terminals to blacken during the sulphur point experiments. It was thus at first thought undesirable to touch the lead connections once the thermometer was attached, especially as it was supposed that if error were at all likely to arise in so obvious a way, provision would have been made for detecting and eliminating it. Thus the matter did not receive consideration until 1897, when the erratic nature of some of the results drew attention to the fact that the leads were becoming worn near the ends.

They were repaired by the Instrument Company, and on their return in September, 1897, a large discontinuity was observed in the values of R_0 in the platinum thermometers. I then found that interchanging the leads caused a difference of fully 1° in the reading. This seeming unsatisfactory, the leads were readjusted by the Instrument Company, and we assured ourselves of their approximate equality, without, however, attempting to measure the exact difference. As the leads remained satisfactory to the eye, no more attention was given to the matter until the autumn of 1898, when a conspicuous drift appeared in the values of R_0. Experiment then showed that the difference between the leads was liable to large fluctuations, accordingly new leads were got from the Instrument Company in December, 1898. These had been adjusted with great care, and on their arrival at Kew they were found to be very nearly though not exactly equal.

Table III gives a brief synopsis of our experience with the leads. The quantity tabulated is the excess of the box reading obtained—the thermometer being in ice, steam, or sulphur vapour—when the CC leads were connected to the thermometer over the reading when the PP leads were so connected.

Table III.—Effect of Interchanging Leads.

Date.	Number of experiments.	Thermometers used	Observed differences.		
			greatest	least	mean.
1897.					
Sept. 21	1	K_1	—	—	1°·029
[Leads adjusted.]					
1898					
Oct. 18	6	$K_1K_4K_7$	0°·432	0°·391	0·114
Oct. 19, 20............	8	$K_1K_2K_4K_6$	0·473	0·438	0·453
Nov. 7, 8, 9, 10, 11	6	$K_1K_2K_4K_7$	0·550	0·494	0·517
Nov. 26	1	K_7	—	—	0·257
Nov. 28, 29, Dec. 8	4	K_7	0·376	0·366	0·372
[New leads.]					
Dec. 8, 12, 13	4	$K_1K_6K_7$	0·026	0·018	0·023
1899.					
Jan. 24, 25............	4	$K_1K_5K_6K_7$	0·033	0·011	0·020
May 10, 17............	2	K_1K_4	0·026	0·013	0·020
June 27	1	K_2	—	—	0·032
July 7, 10	7	K_1K_5	0·062	0·045	0·054
Sept. 14, 15, 18, 19, 20..	8	$K_1K_2K_4K_5K_6K_7$	0·044	0·023	0·036

Each experiment consisted of at least two readings with each arrangement of the leads.

The resistance of one pair of the new leads was recently found by Dr. Harker to be approximately 0·07 ohm at atmospheric temperature, *i.e.*, about 7 per cent. of the fundamental interval of one of the ordinary thermometers. Thus a change of one-thousandth part in the resistance of one of the leads would answer to an alteration of about 0·014 in the quantity tabulated in Table III, or to an apparent shift of 0°·007 in the zero of an ordinary thermometer.

There are various suggestive features in the table. Between October 18 and November 11, 1898, the difference between two leads, immaculate to the eye, altered steadily in one direction by an amount equivalent to a change of 0°·05 in the zero of a platinum thermometer. Then, without apparent cause, there occurred a sudden alteration in the opposite direction, equivalent to a change of 0°·12 in the zero. After this very erratic behaviour the leads seem to have remained practically constant for ten days.

Next we come to absolutely new leads of the best construction. No one, I think, can question the reality of the differences between the results of December, 1898, July, 1899, and September, 1899. But with the occurrence of differences of the size shown during 1899, we must anticipate errors of the order of at least 0°·01 or 0°·02 in the absolute readings, unless adequate provision is made for eliminating this source of uncertainty.

Changes in the leads have unquestionably been the principal cause of the apparent changes in the zeros of the platinum thermometers during the investigation. These changes have added considerably to the difficulty of working up the results.

The difference between the proportional arms was recently found by Dr. Harker to be less than 1/6000 of either resistance. Doubtless the resistances were originally made as nearly equal as possible, so that there is a strong presumption that if any differential change has occurred it has been small. There is, however, unfortunately no direct evidence bearing on this point.

§ 10. It is only proper to remark that from some points of view *slow, regular* changes in the relative resistances either of the leads or of the proportional arms are not of primary importance. Such changes, at least when small, are equivalent, the one to the addition of a constant quantity to the observed resistances, the other to the multiplication by a constant factor, so long as the measurements dealt with cover only a short time during which no sensible variation occurs. Supposing sufficiently numerous observations made of the resistances R_0 and R_1 in ice and steam, it is clear that the platinum temperature *pt*, given by

$$pt = 100\,(R - R_0)/(R_1 - R_0),$$

would remain practically unaffected.

But, on the other hand, the necessity of frequent zero point observations is one of the drawbacks most frequently dwelt upon in the case of mercury thermometers, and it is only fair to recognise that they do not in this respect necessarily suffer by comparison with platinum thermometers.

Coil Changes.

§ 11. The box was originally calibrated at Cambridge by Mr. E. H. Griffiths, who referred everything to a mean box unit based on the eight coils H to A. Since the box came to Kew Observatory it has been thrice calibrated, with the aid of a convenient apparatus designed by Dr. Harker. The observations were made on the first of these occasions by Dr. Harker; on the other two occasions mainly by Mr. W. Hugo, Senior Assistant at the Observatory. I took a certain number of check observations during the two last calibrations, and made all the calculations necessary to construct the correction tables based on these and on Dr. Harker's calibration. In doing so, I followed Mr. Griffiths' procedure, except in some minor details.

The results of the four calibrations were as follows :—

Table IV.—Coil Values in Terms of Mean Box Unit.

Coil	Nominal value	Calibration I (Sept 1895).	II (May, 1897).	III (July, 1897).	IV (Mar.–Apr., 1899).
A	640	639·941	639·973	639·980	639·975
B	320	320·169	320·003	320·000	320·090
C	160	160·062	160·023	160·025	160·028
D	80	80·010	80·001	79·991	79·991
E	40	39·968	40·037	40·028	40·023
F	20	19·963	20·111	20·115	20·115
G	10	9·945	9·760	9·768	9·774
H	5	4·942	5·002	5·005	5·004
I	100	99·957	99·989	99·975	99·426
Mean 1 cm. bridge wire		0·995	0·993	0·994	0·995

The considerable differences between the results of Calibrations I and II are probably mainly due to changes attending the reorganisation of the box in the spring of 1897. Coil I broke at one end, and had to be resoldered just before the last calibration.

It is improbable that the algebraic sum of the resistances of the eight coils H to A was unaffected by the box changes in 1897, and thus the mean box units in Calibrations I and II were almost certainly slightly different. The data obtained with the thermometers, as will be seen later, suggest a slight increase at this time in the mean box unit; and this is, at least, consistent with the apparent change in the bridge-wire resistance.

We should rather expect the resistance of the bridge wire to increase, especially near its centre, through constant rubbing by the contact piece and the figures obtained in Calibrations III and IV somewhat favour this view Too much significance ought not, however, to be attached to the small differences apparent

Some of the differences in the coil values in Calibrations II, III, and IV may be experimental errors, but the changes shown in the case of, at least, E and G are I think, too large to be accounted for in this way

§ 12 Perhaps the clearest evidence of the reality of coil variations is that afforded by an examination of the thermometric results obtained with the different coil combinations It would occupy too much space to go into details so I merely record in Table V the mean differences between the values of R_0 R_1 and R_s (resistance in sulphur vapour) obtained with the two coil combinations used during different specified epochs The unit in the table is 0 01 mm of bridge wire, answering approximately to 0 001 C with the ordinary thermometers

In the final means equal weight is allowed to each observation, so that some thermometers exert more influence than others

During some of the epochs—especially 1895 and the first part of 1899—the data with any one thermometer were very scanty Again, it must be remembered that the part of the bridge wire at which readings are taken differs according to the thermometer used, and also varies for any one thermometer according to the temperature of the coils, the position of the bridge centre, and the difference between the leads It is also different in the ice, steam and sulphur point observations, fluctuating considerably in the latter two cases with the barometric pressure Thus the fluctuations in the table amongst the results for a common epoch are due to many causes

§ 13 It is I think most instructive to start with Calibration III made in July, 1897 During the rest of that year the results from the two coil combinations show almost perfect agreement, in 1898 the results drift apart, and the drift is accentuated in 1899 prior to Calibration IV Again, for some months after that calibration there is an excellent agreement, though a tendency to drift soon manifests itself

Calibration II seems less successful, but it was made by an observer different from the one who took the readings on which the results in Table V are based A different standard of plug tightness does not influence all plugs alike, and in my experience the personal equation in this matter requires to be reckoned with

The data from March 12 to 19, 1897, were so outstanding that they are given separately The exposed parts of the box had, I believe, been cleaned shortly before, and conceivably one of the coil supports may have got a knock There was, however, no suspicion of this at the time This fact emphasises the necessity of a constant outlook for possible changes

Table V

Epoch	Calibration	Mean excess of readings with combination C D F (I) over readings with combination C D G H (I)													Mean for all both ice and steam	Mean excess of reading with combination A E over reading with combination A F G H in sulphur vapour						Mean for all
		In ice						In steam														
		K_1	K_2	K_3	K_4	K_5	K	K_1	K_2	K_3	K_4	K_5	K_6	K		K_1	K_2	K_3	K_4	K_5	K	
1895	I		− 9	−28	−46				−18	− 8	+22		+28		−10		0	−18	−22			−13
1896	I	−11	−12	−14	− 9	−10	−26	−13	− 7	−12	− 7	−11	−12	−20	−11	+14	− 3	+11	+16	+ 5		+ 8
1897 Jan Feb	I		−23	−10	−16	+ 9	− 8		− 2					− 2	− 6							
1897 Mar 12 19	I	+41	+46	+6[illegible]	+55	+60	+46	+54	+62	+41	+44	+4[illegible]		+54	+50							
1897 June	II	+22	+26	+15	+ 5	+12	+ 8	+16	+18	+24	0	+ [illegible]	+10	+ 6	+13	+28				+36		+32
1897	III	+ 3		+ 3	+ 4		0	+ 4		− 3	+ 3		− 3	−10	− 1	+ 4		0	− 8		− 1	− 1
1898	III	− 5		−10	−14		−17	− 3		−13	−13		−30	− 8	−12	+ 1		−20	−26		−19	−17
1899	III	−10		−15	−29	−34	−49	−14		−18	−32	−19	−51	−50	−28	− 8		−48	−45	10	− 6	−23
1899 May—July	IV	+ 3	− 1	+ 8	+ 2	+ 8	− 3	+ 6	+ 1	+14	+ 7	+ 4	− 6	+ 7	+ 4	+10	− 4	−2	+ 1	+21	+ 4	+ 2

A complete calibration is a laborious process, and its frequent repetition would add seriously to the work entailed by platinum thermometry If however the results in Table V are normal it would appear that frequent calibrations can hardly be avoided in the case of physical work of the highest accuracy

Faulty Action of the Contact Piece

§ 14 Parallel to the real bridge wire in the Kew resistance box runs an exactly similar wire connected to the galvanometer and the contact piece works by pushing down a short cross wire so as to span the interval between the wires In January 1898 it was noticed that mere moving and resetting the contact piece might alter the reading, and I found that the pressure to which the cross wire was exposed when contact was made—always acting on the same part of its surface—had cut a groove in it The Instrument Company put this to rights but the phenomenon had repeated itself by January 1899 though to a smaller extent On that occasion the Instrument Company made an alteration which it is hoped will prevent the recurrence of this trouble It is difficult to keep the two parallel wires equally tight and exactly at the same horizontal level thus the cross wire may bear unduly heavily on one wire before it makes good contact with the other This defect would be of little consequence in an open scale bridge wire but in the Kew box it produced when at its worst uncertainties of the order 0 04 or even 0 05, in individual readings Of course this merely tended to introduce irregularity in the readings and supposing a number of observations taken could hardly simulate a change in a thermometer

Shift of the Bridge Centre

§ 15 By the bridge centre I mean the vernier reading when a balance is made with all the plugs in their holes the platinum thermometer and compensator resistances being cut out by short-circuiting straps

The original departure 0 006 in this centre from zero on the scale was given as a fixed correction in the first calibration table and as no provision existed for determining the centre I did not for some time properly appreciate the situation My attention was first roused by a sudden apparent discontinuity in the values of R_0 in the spring of 1897, for which the only apparent cause was a cleaning up of the box The Instrument Company then supplied two short-circuiting straps, to be put across the CC and PP box terminals, and since then determinations of the centre have been made at the beginning of each observation day, and usually at intervals throughout it

Soon it became apparent that during a day's observations the bridge centre is apt to drift towards the minus side of the scale, the same side as it goes to when the plugs are dirty and have an increased resistance. The amount of the drift is very variable. One day it may be negligible, while the next day it may be as much as 0°·04; it is occasionally more than this. The phenomenon is probably partly due to the fact that frequent pulling out and putting in of the plugs seems inclined to produce the grey powder already referred to. It may also arise from the observer's standard of plug tightness falling off as he becomes tired, or from asymmetric heating of the box through the proximity of the observer's person to the minus side of the scale. There are probably various influences at work, some of which may be peculiar to the particular box.

One cannot be perpetually taking bridge centres, so that even the more recent observations are exposed to some uncertainty from this source.

The practice of regularly taking bridge centres was not introduced until 1897, and it is of course possible that there was little or no occasion for it in the original box with brass plug holes.

Thermo-electric Currents.

§ 16. The principal seat of thermo-electric currents seems to be the junctions at the head of the thermometer; and it is desirable to shield the head as much as possible from heated air or vapour. When a Griffiths key is used these currents are not necessarily a source of error, but they tend to increase the difficulty of taking readings. Originally it was supposed that the use of a Griffiths key rendered further care unnecessary, and no commutator was used until after the restoration of the box in 1897. As the Instrument Company then transferred certain terminals to the key-board, it is *possible* that the original box suffered but little if at all from the source of trouble now to be described.

When the commutator was introduced the following phenomena were observed. When the day's observations began the readings with the current d and r (direct and reversed) were usually about the same. After a few minutes' observations the difference between the d and r readings began to increase, and nearly always in one direction. After a little the difference usually assumed a fairly constant value, but considerable fluctuations might occur, especially if the temperature of the room was rendered unsteady. On the advice of Mr. W. N. Shaw the thermo-electric key was enclosed in a padded box. This has decidedly diminished the evil, but it still appears expedient to take readings with the current both ways. Table VI shows the state of matters typical before and after the introduction of the protecting box.

When the thermo-electric effect is not eliminated the error in the reading is $(d \backsim r)/2$.

Supposing $d - r$ to remain constant through the ice, steam, and

Table VI

Without protecting box			With protecting box		
Thermometer	Hour	Difference $r-d$	Thermometer	Hour	Difference $r-d$
	h m			h m	
K_1	11 45	0 000	K	11 2	0 000
	11 54	0 064		11 35	0 003
	12 13	0 094		12 20	0 017
	12 39	0 106			
			K_2	12 50	0 016
K_3	2 56	0 086		2 31	0 020
	3 30	0 105			
			K	2 50	0 014
K_4	3 41	0 107		3 33	0 018
	4 20	0 121			
			K_4	3 50	0 027
				3 59	0 022
				4 26	0 019

sulphur point observations with a particular thermometer the R_0, R_1, and R_2 would be wrong to the same amount so that the fundamental interval and pt_s (the value of pt answering to the boiling point of sulphur) would be unaffected In practice however we cannot anticipate so favourable a contingency

After some experiments I found that I could at pleasure change the amount and even alter the sign of the $r - l$ difference by heating with the finger or cooling by an air blast one of the terminals of the Griffiths key

Heating due to the Battery Current

§ 17 In the Kew apparatus there is an option of two resistances, viz 100 and 20 ohms in the battery circuit, and our original instructions were to use the 100 ohms when observing with an ordinary thermometer in ice and steam, and the 20 ohms when observing at the sulphur point The object doubtless was to ensure sufficient galvanometer sensitiveness at high temperatures

In an early sulphur experiment I was surprised to find that the reading was about 0 07 higher with the 20 ohms in use than with the 100 There being only a single dry cell in use, I had not anticipated a sensible difference Supposing the cell to remain constant the heating effect would of course remain the same for a given thermometer at any one fixed point, and so would not influence the results immediately in view It seemed, however, inexpedient to trust to this, and we have accordingly employed the 100 ohm resistance in all the regular observa tions, merely using the 20 ohms occasionally to furnish data from which to calculate the heating effect under the normal conditions To understand how this is done we must glance briefly at the theory.

Let E be the E.M.F., R′ the internal resistance of the battery, R the rest of the resistance in the battery circuit. Let r_1 be the resistance in either of the proportional arms, r the resistance in the bridge arm containing the platinum thermometer, whose spiral has a resistance ρ. Then if i denote the current in the spiral, H the heat given to it in unit time, we have

$$H = i^2\rho = E^2 r_1^2 \rho \div [(R+R')(r+r_1)+2rr_1]^2 \text{ .... (1).}$$

The heating of course is gradual, and theoretically it might be possible, by rapid, skilful manipulation of the tapping key, to obtain a balance before there is a sensible effect. In practice, however, this is hardly possible in work of the highest accuracy. Only by the remotest chance does one hit the balance at the first attempt, and, as a rule, the key must be put down a good many times. Also, unless the key is held down a sensible time, a small absence of balance may be overlooked. The weaker however the current, the longer the time before sensible heating exists, and with the 100 ohms in the circuit it seems possible to get a fair balance before the heating effect is appreciable. Thus in comparing platinum and mercury thermometers at high temperatures, where accuracy of the order 0°·01 C. is usually much above what is necessary, the bridge has been regarded as balanced when no movement appears in the galvanometer on depressing the key.

In the fixed point observations, on the other hand, the resistances have been adjusted until no movement appears on releasing the key. Under these circumstances the current has exercised its full heating effect. The platinum spiral is presumably heated to such small extent above the surrounding temperature as is required for its gain of heat from the current to balance the loss by radiation, conduction, &c. We may pretty safely assume that this excess of temperature is proportional to the heat given to the wire, but it must also depend on the specific heat of the platinum, on its radiating and conducting power, and also conceivably on the shape, dimensions, and material of the enclosing tube. I understand that several authorities* have proposed to keep $i^2\rho$ constant in the spiral by suitably altering the battery resistance. This, however, as the above reasoning shows, cannot secure constancy in the temperature excess of the spiral at all temperatures.

§ 18. The application of the formula (1) presents difficulties. We know—at least, very approximately—the resistance r_1 of the proportional arms, and the resistance ρ of the spiral, at any "fixed point" temperature, but it is not customary to measure directly the resistance r It exceeds ρ by an amount equal to the resistance of the leads including the thick platinum wires inside the thermometer tube The mean temperature of these latter wires depends partly on the temperature of the spiral partly on the length of tube immersed, and partly on the temperature of the external air Of course we can measure r on any given occasion, but its fluctuations would be considerable under normal conditions My object being rather to get a fairly exact general idea of what to expect under the varied conditions of normal use than to measure the phenomena with extreme exactness under an arbitrary set of conditions I have accepted for $r - \rho$ a rough approximation good enough for my special purpose

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* For instance, Waidner and Mallory, 'Phil. Mag.,' July, 1899, p. 14.

The internal resistance of a dry cell varies very much according to its freshness I have thus made two calculations in which this resistance R is taken as 0 and as 5 ohms respectively The latter figure actually applies to the later experiments on the relative heating in ice, steam and sulphur vapour while the former answers sufficiently to the case of a new cell

The E M F of a dry cell falls off in time though not to a very large extent and for our present purpose such a change does not concern us We do not need to know the value of H as given by (1) in absolute measure but only the relative values of H with varying ρ and r, but with constant I and r_1

In the following calculations I have supposed $r_1 = 5$—the proportional arms having each a resistance of 5 ohms—and for an ordinary thermometer $\rho = 2\ 6$ in ice, 3 6 in steam and 6 8 in sulphur, while $r - \rho = 0\ 4$ for K_6 we have $\rho = 6\ 45$ in ice and 8 95 in steam The calculated values of $(H/E\,r_1^2) \times 10^7$ are given in Table VII

Table VII —Values of $(H/E^2 r_1^2) \times 10^7$

Fixed point	R	Ordinary thermometers						K_6		
		R = 100	60	40	30	20	Difference 20 and 100	R = 100	20	Difference 20 and 100
Ice	0	38	100	212	357	720	682	—	—	—
	5	34	—	—	—	491	457	37	484	447
Steam	0	41	107	225	375	744	703	—	—	—
	5	37	92	182	286	513	476	35	437	404
Sulphur	0	41	—	—	—	681	640	—	—	—
	5	37	—	—	—	478	441	—	—	—

The most striking features in the table are the comparative smallness of the differences between the supplies of heat at the three fixed points, with a given value of R′, and the relatively large effect of an addition of 5 ohms to the battery circuit when the rest of the resistance is only 20 ohms.

No very serious error would arise from treating the heat supplied as independent of the temperature, at least up to 445° C., so long as there is a large resistance of the order of 100 ohms in the battery circuit; and under these conditions a moderate change in the resistance of the battery itself is not of much consequence.

§ 19. At any one fixed point, as already stated, we may reasonably expect the rise of temperature of the spiral to be proportional to the supply of heat. It is desirable, however, to check this conclusion by experiment. I thus compare in Table VIII the above theoretical results with some experiments made with thermometers K_2 and K_5 at the steam point in June, 1897. In these the additional resistance in the battery circuit—beyond that of the battery itself—was given several values intermediate between 20 and 100 ohms.

Assuming

$$\text{Heating effect/heat supplied} = C, \text{ a constant,}$$

I determined C by equating the observed and calculated values of the difference of the heating effects with the two extreme additional resistances 20 and 100 ohms. This quantity is denoted by $H_{20} - H_{100}$, and a similar notation is employed for the other similar difference effects. The differences in the table are really lengths of bridge wire, but for practical purposes they may be treated as temperatures.

Table VIII.—Heating Effects of Battery Current at Steam Point.

		R'.	$H_{20}-H_{100}$.	$H_{30}-H_{100}$.	$H_{40}-H_{100}$.	$H_{60}-H_{100}$.	H_{100}.	H_{60}.	H_{40}.	H_{30}.	H_{20}.
K_2	Calculated	0	$\overset{\circ}{0}$·165	$\overset{\circ}{0}$·078	$\overset{\circ}{0}$·043	$\overset{\circ}{0}$·015	$\overset{\circ}{0}$·010	$\overset{\circ}{0}$·025	$\overset{\circ}{0}$·053	$\overset{\circ}{0}$·088	$\overset{\circ}{0}$·175
		5	,,	0·086	0·050	0·019	0·013	0·032	0·063	0·099	0·178
	Observed	...	,,	0·100	0·047	0·019					
K_5	Calculated	0	0·185	0·088	0·048	0·017	0·011	0·028	0·059	0·099	0·196
		5	,,	0·097	0·057	0·022	0·014	0·036	0·071	0·111	0·199
	Observed	...	,,	0·097	0·061	0·026					

The resistance of the battery itself was not determined on either occasion; but from the large size of the heating effects compared to those of later experiments with an older cell, I am inclined to suppose that the resistance was nearer 0 than 5 ohms. The values calculated with R′ = 5, however, unquestionably present the closest accordance with experiment. In fact, taking into account the difficulty of the experiments, the agreement in this case is altogether too good, and must be largely a matter of luck. However this may be, there is, I think, every reason to suppose that the values deduced for the heating effects H_{100}, &c., with the additional battery resistances 100, &c., are a very fair approximation to the truth. There seems little doubt that with a fresh dry cell the reading at the steam point is raised fully the hundredth of a degree, even with 100 ohms in the battery circuit. If 20 ohms only were inserted, the heating effect would amount to nearly two-tenths of a degree. In this latter event, an increase of 5 ohms in the battery resistance would lower the observed value of R_1 by about 0°·06 C.

§ 20. We have next to consider the relative values of the heating effects at the three fixed points. These have been determined by a series of special determinations of the values of $H_{20} - H_{100}$. During the majority of the experiments the battery resistance was approximately constant, and not far from 5 ohms. The following table gives the observed values of the ratios

$$
\begin{array}{ll}
 & (\overline{H_{20} - H_{100}} \text{ at steam point})/(\overline{H_{20} - H_{100}} \text{ at ice point}) \\
\text{and} & (\overline{H_{20} - H_{100}} \text{ at sulphur point})/(\overline{H_{20} - H_{100}} \text{ at ice point})
\end{array}
$$

for the several thermometers, as well as the values calculated from Table VII for the ratios of the heat supplies.

Table IX.—Differential Heating Effects at Ice, Steam, and Sulphur Points (found experimentally).

	K_1.	K_2.	K_3	K_4	K_5	K_7	Mean K_1 to K_7	K_6.
Steam/ice	0·77	0·77	0·74	0·75	0·67	0·75	0·74	0·54
" "	Ratio of *heat supplies*..					R′ = 0	1·03	—
						R′ = 5	1·04	0·90
Sulphur/ice....	0·33	0·26	0·17	0·40	0·42	0·29	0·31	
" "	Ratio of *heat supplies*... ...					R′ = 0	0·94	
						R′ = 5	0·97	

The table is based on eighteen experiments comparing the (steam/ice) ratios, and eight experiments comparing the (sulphur/ice) ratios. In most cases the several steam/ice observations with the same thermometer showed a remarkably good agreement, but the results with K_5 and K_7 were less harmonious than the others. The sulphur/ice observations were so few that little weight attaches to the results for the individual thermometers; but the final mean should not be much in error. The highest value of the sulphur/ice ratio observed was 0·42 in the single experiment made with K_5. A considerable number of heating experiments have been made in sulphur, but I have employed in Table IX—as in the case of steam—only those which were made on the same day as a corresponding experiment with the same thermometer in ice. This is necessary on account of possible changes in the resistance of the dry cell.

At the sulphur point the heating effect is only about one-third of what it would be if it depended only on the value of $i^2\rho$ in the spiral, and even at the steam point the heating is less than three-quarters of what it would be on this erroneous hypothesis.

§ 21. If the heating effect due to a given $i^2\rho$ were the same at all temperatures, Tables VII and VIII would justify the conclusion that with 100 ohms in the battery circuit all that is to be feared is a trifling fall, of the order 0°·003, in the apparent zero R_0 as the cell deteriorates. But, according to Tables VIII and IX, the true conclusion is that even with 100 ohms in circuit the heating effect is likely to diminish the fundamental interval by several thousandths, and the ice-sulphur interval by not much less than a hundredth of a degree. The use of 20 instead of 100 ohms in sulphur point observations would largely increase the uncertainty.

The changes in $R_s - R_0$ and $R_1 - R_0$, due to varying heating effect, are in the same direction, so that the effect on pt_s is relatively small, and, in view of the numerous uncertainties existent, I have not thought it worth while to attempt a "reduction to an infinitely small current." It is clear, however, that unless the current can be materially reduced by the employment of a more sensitive galvanometer, the necessity of providing for this reduction must be kept in view in all work of the highest accuracy.

Before quitting the subject, I would remark that the diminution of the heating effect of a given $i^2\rho$ as the temperature rises is only what we should expect from the known increase with temperature of both specific heat and radiating power. It is possible that heating effect experiments with thermometers of other metals than platinum, or with spirals of varying gauge of wire, might prove a useful method for investigating radiating power.

Error in the Temperature Coefficient of the Coils or Differences between the Temperature Coefficients of Different Coils.

§ 22. The standard temperature selected by Mr. Griffiths was 20° C., and he found for the relation between the resistance r_τ of a sample of the coil wire at temperature τ, and its resistance r_{20} at the standard temperature,

$$r_\tau = r_{20}[1 + 0{\cdot}00026(\tau - 20)] \quad \ldots\ldots\ldots \quad \ldots\ldots \quad (2).$$

All temperature corrections have been deduced by (2) since the box came to Kew Observatory.

Suppose after the calibration corrections are applied that the observed box reading makes ρ the resistance of a certain platinum thermometer when the coils and bridge wire are all at temperature τ, then, referred to the mean box unit at 20° C, the true resistance is by (2)

$$\rho\,[1+0\ 00026\,(\tau-20)],$$

and thus the proper temperature correction is

$$0\ 00026\,(\tau-20)\rho$$

Now suppose R_t the true resistance at t of a platinum thermometer whose true resistance at 0 is R_0, and whose fundamental interval is I Then the platinum temperature is

$$pt = 100(R_t - R_0)/I \qquad (3),$$

and corresponding to any correction (or error) ΔR_t, arising from a cause other than errors in R_0 or I, we have for the correction (or error) in pt the formula

$$\Delta pt = \Delta R_t(100/I) \qquad (4)$$

Supposing (2) correct, and the box at temperature τ, we must have in order to balance the bridge

$$R_t = \rho\,[1+0\ 00026\,(\tau-20)] \qquad (5)$$

If then Δpt is the correction to pt arising from our application of a temperature correction to the box readings, we have

$$\Delta pt = (100/I)\rho\times 0\ 00026\,(\tau-20) \qquad (6),$$

where ρ, like I, is measured in mean box units

For one of the ordinary Kew thermometers the following are sufficiently near values for purposes of illustration —

	Ice point	Steam point	Sulphur point
ρ =	258	358	678

Corresponding to these representative values, with I = 100, we have

	Ice point	Steam point	Sulphur point
Δpt =	$0\ 067\,(\tau-20)$	$0\ 093\,(\tau-20)$	$0\ 176\,(\tau-20)$

Using Callendar's formula,

$$t - pt = \delta[(t/100)^2 - t/100] \qquad (7),$$

we have for the small increment Δt on the air scale corresponding to a small increment Δpt on the platinum scale

$$\Delta t = \Delta pt - [1 - 10^{-4}\,\delta(2t-100)] \qquad (8)$$

Assuming for the sulphur point on the air scale Callendar and Griffiths' value, $t_s = 444°·53$, then in the Kew thermometers, as will be seen later, $\delta = 1·5$ nearly, and we have approximately

$$\left.\begin{aligned} &\text{At the ice point } \Delta t = \Delta pt \times 0·985 = 0·066\,(\tau - 20), \\ &\quad\text{,, steam ,, } \Delta t = \Delta pt \times 1·015 = 0·094\,(\tau - 20), \\ &\quad\text{,, sulphur ,, } \Delta t = \Delta pt \times 1·134 = 0·200\,(\tau - 20) \end{aligned}\right\} \dots \quad (9).$$

§ 23. Supposing that we aim at an accuracy of 0°·001 C., and are able to keep our coils within 1° C of the standard temperature, then it will suffice to know our temperature coefficient correctly to 1 part in 66, to 1 part in 94, or to 1 part in 200, according as the "fixed point" concerned is the ice, steam, or sulphur point.

Accuracy to 0°·001 C. at the boiling point is not a very extravagant aim, and it thus appears desirable to know the temperature coefficient correctly within 1 per cent. unless we can keep nearer than 1° to the standard temperature. From correspondence with Mr. Griffiths, I found that he did not anticipate so high a degree of accuracy as this in his determination of the temperature coefficient, and as a matter of fact two fairly complete experiments by Dr. Harker have given values from 0·00025 to 0·000245 for combinations of several of the actual box coils.

The allowance of a departure of 1° C. from the standard temperature is a narrow one, except under specially favourable conditions. A good thermo-regulator, no doubt, can control the gas supply with considerable accuracy, even under unfavourable conditions; but what the thermo-regulator controls is its own temperature. That temperature is probably in general close to the mean temperature of the immediately surrounding water. But supposing, as may happen in the platinum-thermometer room at Kew, that the air temperature is only 14° one day and 19° the next, then the temperature of the coil chamber will be much higher on the second day than on the first, supposing the thermo-regulator to have been left untouched. One cannot tell in advance what is going to happen during the night, and if one finds in the morning the box thermometer indicating 17° and attempts to bring it up to 20°, then either a long time is wasted in waiting during a very gradual rise, or else one is very uncertain as to the uniformity of the temperature in the coil chamber.

Again, in summer the room may be at 22° or 23°, or more, and the only simple way of keeping the coil chamber at 20°—viz., constant supplies of cold water in the bath—would probably introduce uncertainties greater than those it removes.

On the whole, experience at the Observatory suggests that it is best to keep the box nearly at the temperature of the room, so that seasonal* variations of τ between 17°·5 and 22°·5 have been common. Supposing

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[*] In the course of a single day's experiments the temperature changes were usually only a few tenths of a degree.

$\tau - 20 = 2°\cdot 5$, an error of 5 per cent. in the temperature coefficient would introduce the following errors in t —

Near 0° C.	Near 100° C.	Near 445° C.
0·008	0·012	0·025

The above remarks suppose that we are concerned, as in the Kew experiments, with the *absolute* value of the platinum thermometer resistances, and have to do with observations taken on different days. In ordinary thermometric work uncertainties in the temperature coefficient would be much less serious, supposing one to determine the fundamental interval daily and to combine together only such observations as are taken on the same day. Under such circumstances it would not matter how far removed from 20° the temperature was, or how much in error the temperature coefficient was, so long as the temperature of the coils was strictly constant. Unless, however, the room containing the platinum thermometers is kept at a constant temperature artificially or is exceptionally situated, its diurnal range of temperature will seldom be inappreciable.

Another source of uncertainty is the possibility of differences between the temperature coefficients of the different box coils, or between those of the two proportional arms. Such differences have not been observed at Kew, but there has been hardly any direct experiment, and the comparison of the results from two coil combinations, such as CDF and CDGH, in which only the small coils are different, does not afford a delicate test.

As a difference between the temperature coefficients of the proportional arms was suggested by the Cambridge Instrument Company as a possible explanation of some phenomena (due, however, to some other cause), it is presumably a contingency which should not be disregarded.

Error in the temperature coefficients, it should be noticed, would only introduce irregularity into the results of the Kew experiments. It might conceivably introduce an apparent seasonal variation in R_0 or in the fundamental interval, but could hardly simulate a true secular change.

Change of Zero or other Error in the Mercury Thermometer inside the Coil Chamber.

§ 24. An error of 0°·1 C. in the estimated temperature of the coils affects the calculated temperature in the case of one of the ordinary Kew platinum thermometers by 0°·007 at 0° C., 0·009 at 100° C., and 0°·020 at 445° C.

Thus for accuracy of the order 0°·001 at 100° C. or 0°·002 at 445° C., the temperature of the coils must be known to 0°·01 C. An ordinary uncalibrated mercury thermometer, even with an open scale, cannot be relied on to give temperature differences to this degree of accuracy under the most favourable conditions.

The thermometer in use in the Kew box is a good calibrated one, and errors in its graduation are unlikely to exceed 0°·01. It is, however, subdivided only to fifths of a degree, and individual readings cannot claim an accuracy of 0 01.

Fortunately, errors in reading merely introduce irregularities in the results, and so are not of fundamental importance in the present inquiry.

A more subtle source of trouble is the secular change of zero, normal to mercury thermometers. A rise of 0°·1 in the zero—which is not very much in excess of what actually occurred during the four years occupied by the experiments—would, in fact, exactly simulate a rise of 0° 007 in the value of R_0.

In the ordinary use of platinum thermometers, at least when there are moderately frequent observations of R_0, the circumstances are different. The slow secular change of zero in the mercury thermometer is then of little moment, while great importance attaches to the accuracy of the individual readings.

Failure of the Box Mercury Thermometer to give the True Temperature of the Coils and Bridge Wire.

§ 25. If the room temperature is below 20° C., and artificial heating is employed, we may have a practically stationary reading on the box thermometer, and yet find it differing by several tenths of a degree from a second thermometer whose bulb is at a different level in the coil chamber. Under such circumstances the temperature inside the coil chamber also varies with the distance from the side or end walls.

The coils are of various shapes and diameters, some coming much nearer the top and bottom of the coil chamber than others, and they are necessarily at different distances from the ends. Thus, under the conditions specified above, different coils may possess different mean temperatures, and the temperature given by the mercury thermometer may differ sensibly from that of any one of the coils. Even with no artificial heating of the box, the temperatures at different parts of the coil chamber may differ by several tenths of a degree, when the room temperature is changing moderately fast. In such a case the presence of water in the protecting tank seems a positive drawback, as it makes the temperature of the coil chamber lag behind that of the rest of the room.

When the temperature of the coil chamber is altering, whether through artificial heating or otherwise, the various coils doubtless alter their temperature at different rates, and unless the mercury thermometer is specially chosen it may have a greater or a smaller lag than any one of the coils. The mercury thermometer used in the Kew box, like most open-range thermometers, contains a considerable quantity of mercury, and when the temperature changes fast it lags very sensibly behind the coils. For instance, in some special steam point observations with K_1, when the recorded box temperature rose 2°·5 in twenty minutes, the *corrected* bridge-wire reading fell about 0°·06, instead of remaining practically constant as it ought to have done. The figures really show that the temperature of the box thermometer had lagged about 0°·7 C. behind that of the coils in use. This of course was an extreme case, and under ordinary experimental conditions the difference between the temperatures of the box thermometer and the coils is hardly likely to exceed 0°·3, and is probably seldom half this. Necessarily, however, a good deal depends on the depth of the bulb of the thermometer inside the coil chamber, and any alteration of that depth should be carefully avoided.

§ 26. The temperature of the bridge wire is open to much greater uncertainty owing to its relatively exposed position. The whole bridge wire resistance in the Kew box is fully 30 box units, and if this were all on the platinum thermometer side of the Wheatstone bridge the error arising from the assumption that it possesses the temperature of the box thermometer would be approximately 0·008τ, where τ is the error, in degrees Centigrade, in the temperature assigned.

As τ may easily amount to several degrees, an error of several hundredths of a degree might easily creep in from this cause if one used a large fraction of the bridge wire. Theoretically it is possible to keep the bridge-wire reading within 5 units of the bridge centre,* thus reducing the uncertainty to less than one-sixth of that existing in the extreme case supposed. This, however, has not been compatible with our practice of employing the same coil combinations for all the thermometers of the same pattern.

Even with the drawback of temperature uncertainty it is often advantageous to make a liberal use of the bridge wire. It facilitates finding the balance, and in work such as the comparison of mercury thermometers at high temperatures, where rapid reading is essential, it is a great convenience.

It is thus highly desirable that the bridge wire should be better protected than in the Kew box, and that its temperature should be measured directly.

Insufficient Immersion.

§ 27. Supposing a correct mercury thermometer to be immersed in ice up to the reading − 10° C. on the stem, there is a column of mercury of about 10-degree divisions exposed to the influence of the surrounding atmosphere, whose temperature is, say, 20° C. This emergent column possesses a mean temperature somewhere between 0° and 20°, and so occupies a larger volume than if all were at 0°; on this account alone the thermometer must read too high. The emergent column has a second influence tending in the same direction; it serves, conjointly with the glass tube, as a path for the conduction of heat to the bulb. In high-temperature measurements the error due to a long emergent column may amount to several degrees, the thermometer in this case reading too low. The conduction along the stem is usually much the less important influence of the two.

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* This was the policy recommended by Mr. Griffiths originally.

A platinum thermometer is not wholly free from immersion difficulties. It is not sufficient to have the *spiral* inside the bath or chamber whose temperature is desired, so that the defect in a platinum thermometer is most analogous to the second source of error described in the mercury thermometer.

If, for instance, a platinum thermometer is buried in ice to only a little over the top of the spiral, the tube emerging in a room at 20° C., the heat communicated to the spiral through the surrounding air or through the connecting wires raises the reading appreciably. Similar insufficient immersion in the hypsometer sensibly lowers the reading.

It was difficult to settle on a suitable basis for comparing the effects of insufficient immersion in the different platinum thermometers. The spiral is wound on a mica frame, and usually one of the small mica discs which hold the wires apart is situated immediately above the top of the frame. When this occurred I have taken the "bulb" of the thermometer as extending to the first mica disc; but in one case, where the interval between the first disc and the frame was considerable, I took instead the top of the frame itself. As a rule, the spiral stops short of the top of the mica frame by about $\frac{3}{4}$ cm.; so the definition is at least a lenient one to platinum thermometers.

The tubes of K_5, K_6, and K_7 being of transparent glass, the length of the "bulb" was visible from outside; but in K_1 and K_4 the tubes had to be taken off to allow of the requisite measurements being made.

In the immersion experiments in steam the thermometers were carried in the usual way by a cork, about 3·25 cm. long, fitting the neck of the hypsometer, and the immersion was counted up to the *lower* face of the cork. Thus fully 3 cm. of the tube immediately above the portion exposed to the steam was protected from direct cooling by the air.

§ 28. Table X gives particulars as to the results obtained. It includes the errors observed with the various immersions, measured on the hypothesis that an immersion of "bulb" + 10 cm. was in all cases sufficient. It also gives the lengths of the "bulbs," and the mean temperature in the room during each series of experiments with each

Table X.—Errors due to Insufficient Immersion.

Thermometer.	Temperature of air.		Length of "bulb."	Ice experiments immersion.					Steam experiments immersion.				
	Ice expts.	Steam expts.		Bulb + 0.	+1¼ cm	+2½ cm	+5 cm	+7½ cm	Bulb + 0	+1¼ cm.	+2½ cm.	+5 cm.	+7½ cm.
K_1 ……	11°	18°	cm. 5	0°·186	0°·068	0° 033	0° 014	0°·007	0°·216	0° 085	0°·025	0°·000	0° 000
K_4 ……	15	16	4¾	0·555	0·223	0·095	0·012	0 007	0 904	0·423	0·191	0·049	0·001
K_5 ……	16	18	4	0·385	0·224	0·110	0 026	0 004	0·547	0 248	0 103	0·022	0·000
K_6 ……	13	15	6½	0·154	0·105	0·071	0 022	0·009	0·320	0·186	0 095	0·029	0 007
K_7 ……	14	16	4½	0·284	0·167	0·098	0 024	0·007	0·448	0·251	0·134	0 047	0 015
Means…	14	16	5	0 313	0 157	0·080	0 020	0 007	0·487	0·239	0·110	0·029	0 005
Ratios to error when "bulb" only immersed …… ……………				1 :	0·50 ·	0·26 .	0 06 .	0 02	1 ·	0·49 .	0·23	0 06 ·	0 01

thermometer. This last information is of most consequence in the case of the experiments in ice, because the errors must then increase as the temperature of the room rises, and during the immersion experiments the room temperature was much below its ordinary summer value.

Some of the figures are based on two or three complete independent experiments, but others depend on only one experiment. In most cases there were in addition several check experiments, with only two or three different immersions, but I have left these out of consideration in constructing the table. The results cannot claim any very high degree of precision, because it is difficult to secure uniformity in the conditions, more especially when the immersion is in ice. The difficulty is greater the less the immersion. When the "bulb" only is immersed in ice, the reading after attaining a minimum usually rises somewhat The phenomenon seems mainly due to melting of the surface ice, with consequent deterioration of contact between it and the thermometer, but possibly a difference in the circumstances under which the thick platinum leads and the compensator wires cool may possess some influence. In taking the minimum readings, as I have normally done, I have of course, if anything, favoured the thermometers. This has also been done to some small extent in assuming that "bulb" + 10 cm. is in all cases ample immersion. As a matter of fact, an unmistakable difference was observed in the reading of several of the thermometers when the immersion was increased from "bulb" + 10 cm. to "bulb" + 12·5 cm. This difference was not, however, always in one direction, the phenomena in one case distinctly suggesting that cooling an additional 2·5 cm. of the compensator wires had more effect than cooling an additional 2·5 cm. of the platinum leads.

The experiments are not sufficiently varied to justify any too confident explanation of the causes of the large differences between the different thermometers. Thus the pre-eminence of the errors in K_1 might at first sight be attributed to the fact that the tube is of porcelain and of exceptionally large diameter; but K_1 is also of porcelain, while K_6 has a much larger diameter than K_7. Perhaps the most probable explanation is that K_4 is short for its tube, so that the mica frame – which is exceptionally short—only comes to within about 1 cm. of the end of the tube.

Ceteris paribus, we should expect that with a given immersion, the error would vary approximately as the difference between the temperature of the immersed portion of the tube and that of the air of the room, but to confirm or refute this conclusion would require a considerable number of careful experiments in which either the temperature of the bath or that of the room alone is varied, all the other conditions remaining unchanged. We cannot make use of the experiments in ice and steam for this purpose, because the conditions under which the transfer of heat occurs are radically different. We can meantime only note as an isolated fact that the immersion errors were only about 50 per cent. greater in steam than in ice, whereas the difference between the temperatures of the room and the immersed portion of the thermometer was about six times as large in the former case as in the latter.

It may be only a coincidence—but if so it is a very curious one—that in the mean results, both in the ice and steam experiments, the errors diminish in geometrical progression as the immersions increase in arithmetical progression.

An addition of 1·25 cm. (*i.e.*, one-quarter the length of the "bulb") lowers the error almost exactly one-half.

§ 29. Returning to the general question, we see from Table X that the immersion should be at least 10 cm., in addition to the length of the "bulb"; in all, in the Kew thermometers, *at least* 15 cm. (6 inches).

If the whole length immersed is less than 6 cm., the error in an ice or steam point experiment is unlikely to be much under 0°·1 C.; and until the total immersion exceeds 10 cm., the error is likely to exceed 0°·01 C.

In "fixed point" experiments it is usually easy to have an immersion exceeding 15 cm., and the main results dealt with in this paper are, I believe, free from appreciable uncertainty on this ground.

There are, however, other circumstances under which immersion difficulties arise, as for instance in the comparison of mercury and platinum thermometers at high temperatures.

Unless both the material of the bath and the contained liquid are transparent, we must have the divisions of the mercury thermometer which we have to read emergent. But with ordinary stirring it is also essential, except in a specially protected bath, that the bulb of the mercury thermometer and the spiral of the platinum thermometer should be about the same level. These conditions are often inconsistent with sufficient immersion of the platinum thermometer when we are comparing the lower part of the scale of the mercury thermometer. In fact, my attention was first directed to the question of immersion of platinum thermometers through the unexpected result that raising equally the height of K_7 and a certain mercury thermometer in a bath of molten metal lowered the reading of K_7 most.

Another case where insufficient immersion is to be feared is in the determination of melting points of metals such as silver. The crucible supplied for use with melting silver at Kew would, if full to the lip, allow an immersion of only 10 cm. *in all*. As the silver is stirred during part of the experiment, and is too precious a material to splash all over the furnace, the immersion cannot well exceed 9 cm., and may be considerably less.

It is by no means impossible that insufficient immersion may have been accountable for the fact that the mean values of the melting point of silver from experiments by Mr. Hugo and myself (six in all) were only 958°·2 C. with K_1 and 957°·5 with K_2; while Mr. Heycock and Mr. Neville, at Cambridge, found 961°·1 C. with the same sample of silver but a different thermometer. This is 2° above the highest individual observation at Kew Observatory.

Slowness of Platinum Thermometers in Acquiring the True Temperature.

§ 30. When a mercury thermometer is taken from a room at one constant temperature into another room at a different constant temperature, some time elapses before the reading becomes steady,; while, if the temperature of the surrounding medium alters, the thermometer shows a lag which varies with the thickness of the glass walls and the amount of mercury in the bulb.

As we have seen, the naked coils in the Kew resistance box have a smaller lag than an ordinary mercury thermometer, but it is otherwise with platinum thermometers. Here the wire does not come into direct contact with the external medium, and the attainment of the steady state is somewhat slow.

Experiments have been made with some of the Kew thermometers which have been suddenly transferred from a bath about 15° C. into ice, or into a hypsometer in which the water is freely boiling. The interval, in seconds, was observed which elapsed before the bridge-wire reading came to 1°, 0°·5, or other fixed distance of the final stationary position , this position had been found in a preliminary experiment.

In all cases the immersion was similar to that of the ordinary fixed point experiments.

Two mercury thermometers, Nos. 686 and 750, were experimented on in an exactly similar way for the sake of comparison. They are both Kew standards of the following dimensions :—

	Length of fundamental interval.	Distance of zero mark from lower end of bulb	Length of bulb.	Diameter of bulb
	cm.	cm.	cm.	cm.
K.S. 686....	35	21	2·5	0·6
K.S. 750....	32	8	2·2	0·5

I have not thought it necessary to go into full details of the observations. The following table gives the number of seconds required to get to 1° and 0°·5 of the stationary temperature in ice, and to 1°, 0°·5, and 0°·25 of the stationary temperature in steam. It was found that the rapidity of attaining the steady state in ice depended greatly on the tightness of the packing and the moistness. Thus, particulars are given of experiments made with the ice variously treated—

Table XI.

	Ice								Steam.		
	As usual, but not packed tight.		As usual, packed tight.		Freshly moistened		Mixed with a good deal of water				
Thermometer.	1°.	0°·5.	1°	0°·5	1°.	0°·5.	1°	0°·5.	1°.	0°·5.	0°·25
K_1	51	—	38	50	33	44	28	35	37	44	58
K_4	103	132	50	58	44	53	—	—	51	61	—
K_6	—	—	48	61	—	—	—	—	65	77	80
K_7	—	—	31	44	—	—	—	—	43	—	61
K S 686	—	—	9	13	—	—	—	—	9	—	—
K S. 750	—	—	11	20	—	—	—	—	6·5	—	—

Most of the data are based on at least two experiments. The different experiments with any one thermometer in steam were in good agreement. The agreement in the ice experiments was pretty fair when the ice was moistened or was tightly packed; when, however the ice was not freshly moistened and was only loosely packed the results were very variable. The intervals 1°, 0°·5 were generally only approximate, and to elucidate the exact law of cooling or heating would require a more complete investigation. The data are, however, exact enough for general conclusions.

The most striking fact is the extreme slowness of all the platinum thermometers as compared to the two mercury thermometers; and it should be noted that these two thermometers are not of a type intended to be rapid.

To get within 1° of the steady reading, the temperatures of the thermometers had to alter about six times as much in steam as in ice. The times required for these two changes are pretty much alike in all the platinum thermometers, supposing the ice tightly packed, as it usually is in the normal experiments. Thus the approach to the steady state is decidedly slower in ice than in steam, and a longer time must be allowed in the former case than in the latter.

§ 31. Some rough calculations, based on the observations with K_1, summarised in Table XI, point to the conclusion that 3·5 minutes' immersion in ice and 2·5 minutes' immersion in steam would suffice to bring the reading within 0°·0005 of its stationary value in the case of this thermometer. K_4 and K_6 are decidedly less rapid, and I should conclude that ten minutes' immersion in well-packed ice and five minutes' exposure to steam are by no means very excessive allowances in the case of an average platinum thermometer It has been customary at Kew to allow a fully longer immersion than ten minutes in ice before taking the first reading, and I think that in the case neither of the ice nor the steam point observations has there been any sensible error through excessive hurry

At the sulphur point the attainment of the steady state is a tedious process Readings taken with the short-stem thermometers K_3 and K_4 within thirty or forty minutes of their first exposure to sulphur vapour had nearly always to be rejected being conspicuously low One had in fact to allow about an hour with these thermometers Even with the long stem thermometers whether glass or porcelain at least forty minutes exposure to sulphur vapour was found desirable A good deal depends on the gas supply and on the quality of the Bunsen burner used

In some of the sulphur point experiments at Kew it is open to doubt whether the stationary temperature had been absolutely attained but I do not think any serious uncertainty was introduced in this way

Impurity in the Ice used in getting R and Instability in the Method of Treating it

§ 32 The ice employed at Kew Observatory is supplied in large blocks which are washed prior to use As the testing of thermometers in ice is a frequent occurrence the assistants have no lack of experience, and since a planing machine was introduced some years ago the ice as prepared has been very uniform and finely divided The ice employed in the platinum thermometry has been taken from the supply used in the ordinary test work, and as the purity of this is frequently checked by observations with Kew standard thermometers no serious impurity need be feared The check would, I allow be hardly adequate to settle a question of two or three thousandths of a degree, and no doubt for work of the highest accuracy a more stringent test would be desirable Strong confirmatory evidence of the uniform purity of the ice is afforded as will be seen presently by the small variability in the observed values of the fundamental intervals of the thermometers

The method of preparing and moistening the ice seems to exert a small but sensible influence on the depressed zero readings of mercury thermometers, possibly because the amount of the depression must be influenced by the rate of cooling There seems, however, to be very little if any effect on the zero reading of a platinum thermometer, so long as the immersion is ample and the temperature of the room is moderate

Error in the Barometer

§ 33 Ordinary errors of reading, lag, or improper temperature correction would merely introduce irregularities into the calculated boiling points; but an undetected change of zero in the barometer, or an alteration in the method of reduction, would simulate a change in R_1 and R_s, the same for all the thermometers.

After the first few observations the Royal Society's old double tube mercury barometer—repaired some years previously by Messrs. Negretti and Zambra—was set up in the platinum thermometer room, and it has since then been used in all the steam and sulphur point observations. A recent comparison between it and the Observatory standard barometers shows that no certain change of zero has occurred since it was set up. The surface of the mercury in the cistern of the barometer is about 6 inches lower than the surface of the water in the hypsometer, and about 11 inches lower than the surface of the boiling sulphur. No allowance has been made for these differences of level, as they would introduce extremely small and practically constant errors in the two boiling points.

The same reduction tables have been in use throughout, and the constant factor 1·0006 has been applied in the reduction to gravity at latitude 45°. Until standard gravity is more exactly defined, and relative values of gravity more carefully determined, the accuracy of the reduction is open to question, but for the purposes of the present inquiry the uncertainty is quite immaterial

As to the accuracy of the individual barometer observations, I need only say that readings by Mr. Hugo and myself seldom differed by more than 0 001 inch. The instrument has a tube of 0·6 inch in diameter and responds rapidly to alterations of pressure, so that no serious error need be apprehended from lag.

Impurity in the Sulphur, &c.

§ 34. The sulphur made use of in the experiments has all been obtained from Messrs. Baird & Tatlock; it has been produced by Chance's process. Though several supplies have been obtained, at different times, no discontinuity has been observed in the sulphur points; thus, unless our experience has been unduly favourable, sulphur has at least one great merit as a medium for supplying a fixed high temperature. There are, however, certain disadvantages which have introduced some variability into the conditions of experiment. After an experiment the sulphur solidifies in the tube containing it, and the remelting it on a subsequent occasion not infrequently ends in cracking the tube. On one or two occasions a crack went right round the tube, at a considerable height above the bulb, after the sulphur had been melted and experiments were progressing, and on other occasions the tube was found to have cracked a day or two after the sulphur had cooled. Of one batch of tubes, of thicker glass than usual, several flew into pieces after the sulphur had liquefied, and the observer was lucky in escaping with no damage except to his clothes. In consequence of this defect, we have had to employ about a score of tubes, and these have not been strictly uniform in length, thickness, or diameter. We have also had to employ a considerable number of small asbestos cones, of the pattern recommended by Mr Heycock and Mr Neville, and the tightness with which these have fitted the tubes has varied sensibly. Thus the sulphur point observations have been taken under more variable conditions than those at the steam point.

We have not indeed observed any certain consequences to follow any of the minor changes described above, but more careful experiments would be necessary to justify the conclusion that the variations are immaterial. I am, in fact, inclined to think that variations in the length of the asbestos cones or in the tightness with which they fit the tube are sufficiently probable sources of uncertainty to deserve investigation.

Error in the Formula assumed for the Variation of the Boiling Point of Sulphur with Pressure.

§ 35. In another direction there is, I think, little doubt that appreciable error exists, viz., in the reduction of sulphur point observations to the standard barometric pressure. Professor Callendar and Mr Griffiths accept a simple linear relation

$$\Delta t = (p-760)\times 0.082 \qquad (10),$$

where Δt is the increment in the temperature of sulphur vapour, on the air scale, when the barometer is p mm. instead of 760. This is based on some experiments by Regnault, who examined a wide range of pressure, but had so few experimental points that interpolation is very uncertain.

A priori, having regard to the corresponding phenomenon in steam, one would hardly anticipate great accuracy from a linear relation, unless restricted to a very limited range, while in ordinary every day use we must be prepared to cover at least a range of 30 mm. The Kew experiments, having been taken without any special regard to the atmospheric pressure, naturally cover a fairly wide range, and thus afford a favourable opportunity of testing the accuracy of (10). To this end I have carefully reduced all the sulphur point experiments in a uniform way.

Assuming Callendar's formula

$$t-pt = \delta[(t/100)^2-(t/100)],$$

we may, near the sulphur point, conveniently give it the form

$$pt-pt_s = (t-t_s)[1-100^{-2}\delta(2t_s-100)]-100^{-2}\delta(t-t_s)^2 \qquad (11),$$

where t_s and pt_s are the temperatures, on the gas and platinum scales, of the sulphur boiling point at standard pressure

Hence if (10) be correct, we have, *pt* answering to the temperature of sulphur vapour when the pressure is *p* mm,

$$pt_s = pt - 0{\cdot}082(p - 760)[1 - 100^{-2}\,\delta(2t_s - 100) \\ - 100^{-2}\,\delta \times 0{\cdot}082(p - 760)] \qquad (12)$$

Supposing for instance $\delta = 1{\cdot}5$, and $t_s = 444{\cdot}53$ then

$$pt_s = pt - 0{\cdot}082(p - 760)[0{\cdot}88164 - 0{\cdot}000012(p - 760)]$$

For any ordinary barometric range the term containing $p - 760$ inside the square bracket is negligible and so we may take

$$pt_s = pt - 0{\cdot}0723(p - 760) \qquad (13),$$

where the last significant figure is of course uncertain

In actual application *pt* is the observed value of $100(R - R_0)/(R_1 - R_0)$ in a sulphur point experiment taken when the reduced barometer reading is *p* while pt_s is the value calculated from this experiment for the platinum temperature of the boiling point of sulphur at standard pressure, on the assumption that (10) is correct

§ 36 In calculating pt_s I adopted two methods First I took for $R_1 - R_0$ in each *group* of experiments with a given thermometer (a *group* including all the experiments made during one year, or during the time one particular calibration was in vogue) the mean of the observed values of the fundamental interval during the epoch in question Next I used for each sulphur experiment the individual value of $R_1 - R_0$ found from the corresponding ice and steam point observations These observations were usually taken on the same day as the sulphur point observation, or on the previous day I thus obtained two series of values of pt_s In the first, one principal source of uncertainty was largely eliminated, while, in the second, another principal source of uncertainty was largely reduced

The following table shows the results for the longest epoch considered, viz, from September, 1897, to January, 1899, inclusive, covering the time when Calibration III was in vogue During a set of sulphur experiments with the different thermometers the barometric height of course varied somewhat thus the pressures given in the first column of the table are only approximate The break in the table separates the cases where the reduced barometric height was above 760 mm from those in which it was below The results in the last two lines are based on the exact barometric readings observed during the experiments on the individual thermometers —

Table XII.

Approximate barometric height in mm	Values of pt_s using a mean fundamental interval for the group.				Values of pt_s using individual values for the fundamental interval.			
	K_1.	K_3.	K_4.	K_7.	K_1.	K_3.	K_4.	K_7.
	421° +	421° +	421° +	421° +	421° +	421° +	421° +	421° +
776	·61	·81	80	·65	·59	·76	·74	·56
773	·61	·63	·69	·58	·60	·63	·71	·56
769	·43	·60	·58	·52	·52	63	·50	·57
767	·68	·56	·62	·51	·55	·52	·61	·48
766	·63	·72	·69	·65	·62	·69	64	·44
765	·49	·61	·60	·53	·56	·63	·69	·59
763	·41	·48	·49	—	·46	·47	58	—
758	·50	·48	·47	·42	·47	·57	·55	·38
756	·45	·44	·46	—	·53	·44	·47	—
753	·37	·52	42	41	43	·52	·41	·37
751	44	·57	·64	—	·41	·57	·60	—
746	·48	·55	·61	·46	·43	·58	·59	·46
Mean when pressure >760........	·551	·630	639	·573	·557	·619	·631	·583
„ „ „ <760........	·448	·512	·520	·430	·454	·536	·524	·403
Differences..........................	·103	·118	·119	·143	·103	·083	107	·130
Difference per 1 mm.	·0064	·0079	0079	·0060	·0064	·0055	·0071	0073
„ between	769 5 753·5	768·2 753 2	767·8 752·8	768 8 mm. 750 9 „				

§ 37. A similar treatment of the entire number of observations with these four thermometers—viz., twenty-four with K_3, twelve with K_7, and twenty-one with both K_1 and K_4—making use of the individual fundamental intervals only, gave the following results :—

Table XIII.

Pressure	Mean pressures 700 +				Mean values of pt_s, 421 +			
	K_1.	K_3.	K_4.	K_7	K_1.	K_3.	K_4.	K_7
Above 760	67·57	66·66	66·19	68·77	·508	·612	·625	·533
Below	54·08	54·06	54·63	53·91	·468	·485	·526	·402
Differences....	13·49	12·60	11·56	14·86	·040	·127	·099	·131
Thus variation in pt_s per 1 mm. of pressure					0·0030	0·0101	0·0086	0·0088

As we shall see later, the thermometer K_1 has undergone a change. Its pt_s has risen, and the apparent smallness in the variation in its case in Table XIII seems largely due to the fact that amongst the latest experiments there was a preponderance of cases where the barometer was below 760 mm.

§ 38. Tables XII and XIII prove, I think, beyond a doubt that the Callendar-Griffiths reduction formula (10) assigns an appreciably too small value to the variation of the temperature of sulphur vapour with pressure near 760 mm. This is the conclusion to which the results from every single thermometer point, and the phenomena are too conspicuous to be attributed to accident or to experimental errors.

The agreement between the results in Tables XII and XIII is, I admit, not all that might be desired, and any numerical conclusions based on them are perhaps hardly likely to be final. Still I have thought it worth while to see what these conclusions are, proceeding as follows :—

Suppose that the Callendar-Griffiths formula (10) is defective in omitting a term in $(p-760)^2$ and in assigning an incorrect value to the constant multiplier of the term in $p-760$.

Then, supposing as before $\delta = 1{\cdot}5$, we should replace (13) by

$$pt = pt_s + (0{\cdot}0723 + x)(p-760) + y(p-760)^2. \quad \ldots\ldots (14),$$

where x and y must be determined from the experiments. As all the observations had been already worked up applying (10), it seemed simplest to take for data the values calculated in this way for pt_s, determining x and y by least squares from the observed differences between the calculated values and their mean.

I first took the data in Table XII and added to the individual values for pt_s in K_1, K_3, and K the mean of the differences between the values of pt_s found with these respective thermometers and the corresponding values in K_4. As the corresponding values of pt_s in these four thermometers answered to nearly the same barometric pressure, one got in this way a mean pt_s answering very fairly to the mean barometric pressure. Applying formula (14) to the figures so obtained, I obtained by least squares

$$= +0.0063 \qquad y \qquad +0.00018 \tag{15}$$

Next I took all the observations made with K_3 and K_4—excluding one when the asbestos cone fell off—and combining the observations made on the same days got twenty four values of pt_s as given by the formula (10). Combining these two and two in the order of the corresponding barometric pressures, I found from the resulting twelve data by least squares

$$- +0.0087, \quad y \quad +0.00021 \tag{16}$$

For ready comparison I show side by side the formulæ resulting from (15) and (16) as well as those based on the Callendar-Griffiths value. In these formulæ, t_s and pt_s are as before the temperatures on the air and platinum scales of sulphur vapour boiling *under the standard pressure* 760 mm, while t and pt are the corresponding temperatures when the pressure is not 760, but p mm. It is supposed that $\delta = 1.5$ in the formulæ for pt.

Callendar and Griffiths from Regnault
$$\left\{\begin{aligned} pt &= pt_s + 0.072(p - 760), \\ t &= t_s + 0.082(p - 760) \end{aligned}\right.$$

From (15), i.e., by experiments on K_1, K_3, K_4, and K, between Sept 1897 and Jan 1899
$$\left.\begin{aligned} pt &= pt_s + 0.078(p - 760) + 0.00018(p-760)^2, \\ t &= t_s + 0.088(p - 760) + 0.00020(p-760)^2 \end{aligned}\right.$$

From (16), i.e., by experiments on K_3 and K_4 between Oct 1895 and Sept 1899
$$\left.\begin{aligned} pt &= pt_s + 0.081(p-760) + 0.00021(p-760)^2, \\ t &= t_s + 0.092(p - 760) + 0.00024(p-760)^2 \end{aligned}\right.$$

In restricting the second calculation to K_3 and K_4 I was guided by the obvious change in K_1 and by the comparative fewness of the experiments at the sulphur point with K_2, K_5, and K_7. According, however, to Table XIII the variation of pt_s, per 1 mm pressure, deduced from the experiments on K_3, is exceptionally high. Further, the agreement between observed and calculated values in the second calculation was not so good as in the first. I am thus disposed to attach somewhat more weight to the result of the first calculation. In any case I have little doubt that the formula

$$t = t_s + 0.090\,(p - 760) + 0.0002\,(p - 760)^2 \qquad (17)$$

will prove considerably more exact than the formula (10) now in use. Further direct experiments on the point are, in my opinion, desirable.

§ 39. In our previous calculations we have employed Callendar and Griffiths' value, 444.53 C., for the boiling point of sulphur. This is supposed to be measured on the scale of the constant pressure air thermometer. It is about 0.7 C. lower than the value recently found by Chappuis and Harker, employing the constant volume nitrogen thermometer. We do not as yet know enough about gas thermometry to judge of the consistency of the two results, or of the degree of accuracy either can claim. It is thus desirable to know how the previous results would be influenced by a slight change in the accepted value of t_s. Such a change would not affect any formula like (10) connecting t and p, and it would modify formulæ like (13) containing pt only indirectly through change in δ. The change $\Delta\delta$ in δ, answering to a small change Δt_s in t_s, is given by

$$\Delta\delta = \Delta t_s\,[10^4 - \delta(2t_s - 100)] - [t_s(t_s - 100)]$$

Using Callendar and Griffiths' value of t_s we get approximately, when $\delta = 1.5$,

$$\Delta\delta = 0.0575\,\Delta t_s$$

For instance, if $\Delta t_s = 0.7^\circ$ C.,

then $\Delta\delta = 0.04$, approximately.

Constancy or Variability of Platinum Thermometers.

§ 40. Supposing the R_0 of a thermometer, determined in the usual way, to show a distinctly lowered value as time proceeds, the cause might be a change in the leads, or in the proportional arms, or in the box coils, and not a real change in the thermometer itself. There are no certain data as to the original equality of the proportional arms; but as Dr Harker recently found them to differ by only about three parts in 20,000, and as they were, doubtless, made as nearly as possible equal at first, there is reason to suppose that any uncertainty on this head is small.

On the other hand, as shown by Table III., changes in the leads have been large and fluctuating. As no data exist for eliminating this uncertainty in a direct way in the earlier observations, I have based the inquiry into the constancy of the thermometers mainly on an intercomparison of themselves. As the same coil combinations were used for every one except K_6, and as the thermometers have nearly equal fundamental intervals, and were compared in the great majority of instances on the same or on successive days, the differential results are but little exposed to the chief sources of uncertainty.

Before discussing the differential results I shall glance briefly at the absolute data obtained when the leads were interchanged. A summary of these is given in Table XIV, along with results of observations without interchanged leads on October 4 and December 8 and 9, 1897:—

Table XIV.—Values of R_0.

Date.	Leads.	K_1.	K_3.	K_4	K_7.
September 21, 1897....	interchanged	257·756	257 856	257·453	256·708
October 4, 1897.......	as usual	·730	·833	·431	·672
December 8 and 9, 1897	as usual	·750	·827	·420	·672
Mean of experiments between May and September, 1899........	interchanged	·723	856	462	·711

The leads had been adjusted to approximate equality between September 21 and October 4, 1897; we may thus reasonably suppose that the changes experienced prior to December 9, 1897, were small. Also, judging by what was observed on October, 1898, such change as might have occurred would tend to diminish R_0 We may thus regard the results obtained on October 4, and December 8 and 9, 1897, as proving that the observations on September 21 did not suffer from any notable source of error peculiar to the day.

Comparing the results obtained on September 21, 1897, with the mean results for 1899, we obtain a surprisingly close agreement in the case of every thermometer except K_1.

This certainly is pretty strong evidence of constancy in at least three of the thermometers during the last two years. It is of course possible that a common change might have occurred, and that it was masked by a corresponding equal change in the box coils; but as these coils are of a different material, platinum silver, and have been exposed to wholly different conditions, such a contingency is improbable.

§ 41. For the purpose of intercomparison, I have selected as standards of reference K_3 and K_4. These two thermometers are almost identical in pattern, and have been exposed to almost exactly the same treatment, usually being compared in immediate succession to one another. Taking the mean R_0, R_1, and R_s given by these thermometers on each occasion, I have found the difference between these means and the R_0, R_1, R_s of each of the other thermometers. I preferred taking the mean for the two thermometers to taking either alone as a standard, because by doing so, observational errors should be rendered less important Also there was no reason for preferring one to the other The R and R_1 of K_0 have been multiplied by 0 4 to reduce them to approximate equality with the corresponding quantities in the other thermometers, and in this instance the data given are limited to the case where the leads were interchanged The reason for this is that the value of R in K_0 is affected by a given inequality in the leads by no more than the R in K_3 or K_4 so that inequality in the leads would influence such a quantity as

0 4 R (in K) (mean R_0 of K_3 and K_4)

The differences between the R R and R_s of K_3 and K_4 are also given, so that the differences of any thermometer from K_3 and K_4 separately could be at once obtained from the table Such a heading as K_3 K_4 means that the R R or R_s—as the case may be—of K when subtracted from that of K_3 gives the results in the columns of Table XV (p 4)

The data enclosed in the [ ] brackets are I have reason to think affected by experimental error while those in the ( ) brackets are I believe correct but due to exceptional circumstances These data are not taken into account in the mean values Data in the brackets } refer to two independent observations during the same month The blanks in the case of K and K arise from the absence of these thermometers at Sevres and from the changes in the former thermometer during 1896

§ 12 Table XV shows that if any relative change has occurred between K_3 and K_4 since 1895 it must be very small The figures are not however, absolutely inconsistent with a slight reduction in the difference between the two resistances

There is the clearest evidence of a gradual fall in the R_0 and R_1 of K_1 relative to the corresponding quantities in K_3 and K_4 the change being greatest in the case of R_1 On the other hand the R_s of K_1 has apparently risen during the last two years relative to the mean R_s of K_3 and K_4 As the resistance of K_1 at 0 C is intermediate between the resistances of K_3 and K_4 it is impossible to attribute the phenomena to changes in the proportional arms, or in the coils, or to any other external cause I thus see no alternative to the conclusion that in K_1 there has been a diminution in the resistance in ice and in the fundamental interval accompanied by an increase in the ice sulphur and steam sulphur intervals

In its early days K_1 was several times exposed to the temperature of molten silver, but the last exposure preceded 1897 Table XV shows no other certain example of relative change except in the case of K when quite new The first experiment made with this thermometer was on February 12, 1896, by which time the observers had become fairly

Table XV.—Differences between Resistances of Thermometers

Date	K_3 K_4			k $\frac{1}{2}(k_3+K_4)$			K_2 $\frac{1}{2}(K_3+K_4)$			K_5 $\frac{1}{2}(K_3+K_4)$			$0·4K_6$ $\frac{1}{2}(K_3+k_4)$		$\frac{1}{2}(k_3+K_4)$ K		
	R_0	R_1	R	R_0	R	R	R_0	R	R	R_0	R	R	R_0	R	R_0	R	R_0
1895																	
October	0·415	0·670	—														
December	0·493	0·[illegible]	1·[illegible]														
1896																	
February	0·437	·633	[1·960]	—	—	—	—	—		(0·686)	(1·154)	2·46					
March—April	0·419	0·695	1·514	—		—		—		0·685	1·083	2·[illegible]					
May	0·404	0·65	1·3	0·250	0·218	0·149	—			0·658	1·064	2·314					
June	·39	0·667	1·520	0·216	0·[illegible]3	0·1 2				0·643	1·041	2·291					
August	0·398	0·656	1·563	0·141	0·168	0·[illegible]1	—	—	—	0·63	1·090	·351					
October	0·3 8	0·669	—	0· 90	0·15	—	—	—	—	0·629	1·090	—	—	—	0·9 1	1·536	—
Mean for 1896	0·405	0·665	1·530	0·210	0·186	0·111	—	—	—	0· 49	1·045	2·36			—	—	—
1897																	
March	0·397	0·6 3	—	0·128	0·1 2		—	—	—	0·63	1·07	—	—	—	0·945	1· 26	—
June	0·404	0·659	—	0·137	0·118	—	—	—	—	—	—	—			0·950	1·54	—
August	0·448	0·621	1·408	0·193	0·046	0·019	—	—	—	—	—	—	—	—	·97	[1·621]	3·239
September	0·403	0·6 3	1·565	0·1 9	0·037	0·0 3	—	—	—	—	—	—	0·436	0· 19	·948	1·465	3·308
October	0·402	0·680 0·668	1·494	0·10	0·068	0·125	—	—	—	—	—	—	—	—	0·960	1·501 1·529	3·344
December	0·407	0·671	1·505	0·127	0·097	0·1 4	—		—	—	—	—			0·9 1	1·496	3·3 3
Mean for 1897	0·410	0·664	1·498	0·132	0·090	0· 68	—	—		—		—			0·954	1·510	3·316
1898																	
January—February	0·3 1	0·650	1·4 6	0·	0·049	0·0	—	—	—	—	—		—	—	0·929	1·468	3·306
March	0·396	0·664	1·5 0	0·11	0·06	·1	—		—	—	—	—	—	—	0·933	1·440	3·290
April	0·386	0·65	1·4 1	0·09	0· 7	0· 4		—	—		—			—	0·911	1·504	—
June	0·391	0·663	1·649	·061	0·011	0· 0	—	—	—	—	—	—		—	0·961	1·501	3·336
September	0·365	0·654	1·525	0· 4	0·0 2	0·24	—	—	—	—	—	—	—	—	0·954	1·498	3·323
October	0·401	0·6 8	1·533	0·069	0·021	0·133	—		—	—		—	0·407	0·6 1	0·939	1·506	—
November	0·371	0·658	1·514	0·062	0·045	0·1	—		—	—	—	—	—	—	0·945	1·506	—
December	0·420	0·568	1· 93	0·059	0·011	0·	—	—	—	—	—	—	0·405	0·650	0·93	1·501	3·307
Mean for 1898	0·388	0·661	1·540	0·0	0·025	0·1 5	—	—	—	—	—	—	0·406	0·6	0·944	1·497	3·310
1899																	
January	0·389	0·659	1·550	0·041	0· 10	0·330	—	—	—	0·628	1·019	[1·860]	0·406	0·681	0·9	1·472	3·389
May	0·3 9	0·663	1·296	0·0 6	−0·005	0·22	3·568	4·63	8·011	0·610	1·013	2·439	0·415	0·	970	1·6 9 1· 38	3·381
June	0·398	0·659	1·405	0·059	−0·021	0·03	3·564	4·634	8·0 4	0·640	1·042	2·406	0·424	0·593	·95	1·494	3·365
September	0·39 0·400	0·658	1·594	0·056 0·048	0·030	0·2 1	3·568 3·5	4·580	·840	0·680 0·641	1·044	2·34	0·424	0· 02	0·942 0·946	1·475	3·480
Mean for 1899	0·393	0·660	1·486	0·0 6	−0·016	0·230	3·563	4·631	7·97	0·636	1·050	2·413	0·417	·644	0·950	1·4	3·3 9

expert. On this day observations were taken at the ice, steam, and sulphur points, and on the same day observations were also taken with K_3 and K_4. Further, both Mr. Hugo and myself observed, each with two sets of coils, at the ice and steam points, and our observations were in excellent agreement. Thus I think the evidence is almost conclusive as to the reality of a fall in the R_0 and R_1 of K_5, in consequence presumably of its first exposure to the sulphur point temperature. This fall was apparently about 0°·1 in the case of R_0.

This is not the only case in which this phenomenon occurred. Thus on April 24, 1896, the thermometer K_2, which had been freshly remade after a breakage in silver, gave an R_1 of 356·507 immediately prior to a sulphur point experiment, while on April 27, the value found for R_1 was 356·404. There is no reason to suppose that any sensible change in the leads had intervened.

§ 43. Much the clearest evidence of sudden changes arises, however, in the case of exposure to the temperature of molten silver. On December 24, 1895, K_2 was exposed to this temperature for the first time at Kew,* the experiment being immediately preceded and succeeded by steam point observations. It was found that R_1 had changed from 357·535 to 357·014, representing a fall of 0°·52. On the next trial in steam the value found for R_1 was 357·057, but as the R_1 of K_3 and K_4 showed also a slight apparent rise during this interval, the cause was most probably a slight change in the leads. In March, 1896, K_2 broke in silver and was remade. After exposure to the temperature of sulphur vapour on April 24, as already described, it was tried in molten silver on April 27; on which occasion its R_1 fell from 356·404 to 356·050. After a second sulphur point experiment on June 26, when no certain change occurred, K_2 was tried a second time in silver on July 2, and its R_1 again fell, though only by 0°·15. Altogether, between April 24 and July 2, 1896, the R_1 of K_2 showed a total apparent fall of 0°·735, whilst the corresponding mean apparent fall in K_3, K_4, and K_5, as obtained by interpolation, is only about 0°·015. The mean fall in these last three thermometers is doubtless a nearly correct measure of contemporary change in the leads; and we thus conclude that two exposures to the sulphur point temperature and two exposures to molten silver had produced a fall of approximately 0°·7 in the zero of K_2.

The thermometer K_1 had been in molten silver in September, 1895, before it came to Kew. On its first exposure to molten silver at the Observatory—the exposure being as usual preceded and followed by steam point observations—the fall of R_1 was only 0°·08; and on two subsequent occasions the effect, if any, was very small. For instance, on the last occasion the apparent fall was only 0°·009.

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* It was said to have been heated in September, 1895, at Cambridge, to fully as high a temperature.

§ 44. We have next to consider the constancy of the fundamental intervals. Corresponding ice and steam point observations were almost invariably taken in immediate succession to one another, so that ordinary changes in the leads could hardly affect the results. I have divided the data into groups according to the date, and give only the mean results. In the first group there were only two observations and in the last group only three, except in the case of K_1 and K_5, where five observations had been made In the three intermediate groups the means are nearly all based on six observations. A few observations made in the early part of 1897 have been wholly omitted, as the state of the leads was certainly not then satisfactory.

Table XVI.—Mean Values of Fundamental Intervals at different Epochs.

Epoch.	Calibration used	K_1.	K_2.	K_3.	K_4.	K_5	K_6.	K_7.
		99 +	100 +	99 +	99 +	100 +	250 +	99 +
1895	I	..	..	·956	·711	..	·246	
1896	I	812	..	·981	·709	·247	301	·295
Sept, 1897, to Apr, 1898	III	·776	..	·942	·672	..	·157	·265
Apr., 1898, to Jan., 1899	III	·752	..	·945	·671	..	·163	·257
May to Sept., 1899	IV	·712	857	·925	·656	·188	·171	·256

According to the table there is a fall in the fundamental interval of every thermometer between 1896 and 1899 It is probable, however, that this is mainly if not entirely fictitious except in the case of K_1. When the resistance box was altered in the summer of 1897, the coils, as Table IV shows, were slightly disturbed, and in all probability the mean box unit was slightly altered. An increase in this unit of about 35 parts in 100,000 would account pretty satisfactorily for the apparent differences in the results of the second and third epochs of the table.

During the third and fourth epochs nothing happened to the box, and K_1 is the only case where there is any noteworthy difference between the corresponding means in the two epochs.

Between the fourth and fifth epochs the coil I broke, and its resistance was diminished by about 0·66 of a box unit by the resoldering. This fact, and the comparatively small number of observations on which the means are based during the fifth epoch, predispose me to query the apparent falls in the fundamental intervals of K_3 and K_4. In the case of K_1, however, the fall is too large to be explained away.

§ 45. Instead of attempting to trace changes in the ice-sulphur

Table XVII —Mean values of ρt_s [$\equiv(100\ \mathrm{I})(R_s\ \ R_0)$] for Epochs stated

Epoch	Using the Callendar Griffiths formula						Using the formula (17)					
	K_1	K_2	K_3	K_4	K_5	K	K_1	K_2	K_3	K_4	K_5	K
	421 +	421 +	421 +	421 +	421 +	421 +	421 +	421 +	421 +	421 +	421 +	421 +
1895–6	40	39	57	62	57	—	35	33	55	58	51	—
1897	45	—	54	59	—	47	44	—	55	61	—	49
1898	51	—	56	56	—	49	49	—	[illegible]	56	—	45
1899	60	43	54	58	52	44	58	46	52	56	52	43

Table XVIII —Mean values of δ [in Callendar's formula (7)] for Epochs stated

Epoch	Using the Callendar Griffiths formula.						Using the formula (17)					
	K_1	K_2	K_3	K_4	K_5	K	K_1	K_2	K_3	K_4	K_5	K
1895–6	1 510	1 511	1 499	1 496	1 499	—	1 513	1 515	1 500	1 499	1 503	—
1897	1 507	—	1 501	1 498	—	1 506	1 508	—	1 500	1 497	—	1 504
1898	1 508	—	1 500	1 500	—	1 504	1 504	—	1 500	1 500	—	1 507
1899	1 497	1 508	1 501	1 498	1 502	1 508	1 499	1 506	1 502	1 500	1 502	1 508

interval $R_s—R_0$ directly, I pass to the consideration of pt_s and δ, as quantities of more immediate interest The constancy of one of these quantities is involved in that of the other, but each has an interest of its own I have thought it desirable to give the results obtained from formula (17) as well as those obtained by accepting the Callendar Griffiths formula (10)

The thermometer called K in 1896 is wholly different from that so denominated in 1899

With the exception of the 1899 results in K_2, and the 1897 results in K, every mean in the table depends on at least four observations, in some instances eight were available

The existence of a gradual rise in the pt_s of K_1 with corresponding fall in its δ, is placed I think beyond doubt In the case of the other thermometers there is no fluctuation which might not reasonably be attributed to errors of observation or uncertainty in the pressure reduction formula

Differences between the Thermometers

§ 46 A difference between the values of R_0 or I in two thermometers does not necessarily imply any difference between them as measurers of temperature The temperature *pt* is a ratio and so should depend solely on the law of increase of resistance with temperature

If the drawing of platinum wire affects its physical properties we may perhaps expect differences between the temperature relations of wires of different diameters But there is no obvious reason *à priori* why differences should exist between specimens of wire of the same thickness from the same sample of platinum It is clearly desirable to ascertain whether such differences exist and if they do whether they follow any recognisable law This is the more important at present owing to the recent proposal made by Prof Callendar to the British Association to set up standard platinum thermometers made of some one sample of platinum wire

Referring to Tables XVII and XVIII, we see evidence of differences between certain of the thermometers The consistency of the mean values found in different years for pt_s in K_3 and K_4 renders it impossible to ascribe to experimental error the very considerable differences between the values of pt_s in these two thermometers, and in the original K_1 and K_2

§ 47 The evidence afforded by Tables XVII and XVIII as to differences between K_3, K_4, K_5, and K is less conclusive I have thus had recourse to two more sensitive methods of detecting differences

The first of these consists in taking the ratios borne to one another by the resistances at the ice, steam, and sulphur points The taking note of the value of R_1/R_0 was in fact recommended originally by Mr Griffiths as a delicate means of checking the accuracy of individual observations, and the practice was continued for some time after the experiments commenced. The value of these original calculations is, however, prejudiced by the fact that the results are affected by any want of equality between the leads, unless this can be eliminated. I have thus included in the following table only those observations in which the difference between the leads was known and could be allowed for The thermometers are arranged in descending order of R_1/R_0 during the last epoch.

Table XIX.

Thermometer	Mean values of R_1/R_0.			Mean values of R_s/R_0
	Sept 21, 1897. (Cal III)	Oct, 1898, to Jan, 1899 (Cal III)	May—Sept, 1899 (Cal IV)	May—Sept, 1899. (Cal IV).
K_5	—	1·38792	1·38787	2·63527
K_6	1·38771	1·38777	1·38774	—
K_3	1·38731	1·38759	1·38751	2·63331
K_4	1·38707	1·38714	1·38711	2·63165
K_1	1·38709	1·38706	1·38691	2·63112
K_7	1·38670	1·38661	1·38664	2·62937
K_2	—	—	1·38610	2·62709
Means, excluding K_6			1·38702	2·63090

The last figure is retained mainly to give an idea of the concordance between the several results for the same thermometer. Without realising this, one is not in a position to judge of the weight to be attached to the apparent differences between the different thermometers.

It will be observed that K_1 alone has altered its relative position since September, 1897. Again, it will be noticed that the order of R_s/R_0 is the same as that of R_1/R_0. But for this fact we should have found larger differences between the values of pt_s and δ in the several thermometers.

§ 48. The above table will, I think, be considered pretty conclusive as to the reality of the differences between the different thermometers. I shall, however, also give the results of the second method of attacking the problem, as they possess an independent interest.

If two platinum thermometers differ only in the absolute resistance of their spirals, the difference may be ascribed to a definite portion of the wire forming the spiral of higher resistance. Thus, in such a case, the *difference* between the resistances should increase with the temperature in precisely the same way as the total resistance of either thermometer. Consequently, if the platinum wire in the several thermometers were identical, the differences between the resistances of any two of them at the steam point and at the sulphur point—or the differences between any one and the mean of any two others—should bear to the difference of the resistances in ice constant ratios. And according to Table XIX the values of these two constant ratios should be approximately 1·387 : 1 and 2·631 : 1.

Table XX shows what the ratios actually are in the cases examined. In treating K_6 I utilised only those observations in which the difference between the leads could be allowed for. The meaning of the results will be rendered clear by an example. The mean of the differences between corresponding observed values of R_1 in K_3 and K_4 during 1896 was 0·665, while the mean of the differences between the observed values of R_0 was 0·405; the ratio of these two quantities is 1·64, and it is given under the heading R_1/R_0. This is here used as an abbreviation for

$$(R_1 \text{ in } K_3 \text{ less } R_1 \text{ in } K_4) \div (R_0 \text{ in } K_3 \text{ less } R_0 \text{ in } K_4).$$

Table XX.

Epoch.	K_3-K_4.		$K_1-\frac{1}{2}(K_3+K_4)$		$K_2-\frac{1}{2}(K_3+K_4)$		$K_5-\frac{1}{2}(K_1+K_4)$	
	R_1/R_0.	R_s/R_0.	R_1/R_0.	R_s/R_0	R_1/R_0.	R_s/R_0	R_1/R_0.	R_s/R_0.
1896	1·64	3·78	0 89	0·53	..	..	1·61	3·65
1897	1 62	3·64	0·68	0 67	..	..	1·61	..
1898	1 70	3·97	0 35	2 57	..	..	..	..
1899	1·68	3·79	−0·20	3·93	1·30	2·24	1·62	3·79

Epoch.	$\frac{1}{2}K_6-\frac{1}{2}(K_3+K_4)$	$\frac{1}{2}(K_3+K_4)-K_7$		K_2-K_5.		K_2-K_7.	
	R_1/R_0.	R_1/R_0.	R_s/R_0.	R_1/R_0.	R_s/R_0.	R_1/R_0.	R_s/R_0
1896	..	1·57	..	..	..	..	..
1897	1 63	1 58	3·48	..	..	..	..
1898	1 64	1·59	3·51	..	..	..	..
1899	1·67	1·57	3·50	1·23	1·90	1·36	2·51

In only one case, K_2-K_7, are the ratios even approximately equal to the mean values given by Table XIX, viz., 1·39 : 1 and 2·63 : 1. Even in that case the differences are too big to ascribe to experimental error, because the R_0 of K_2 exceeds that of K_7 by fully 4·5 box units.

In the case of $K_3 - K_4$, and of $\frac{1}{2}K_6 - \frac{1}{4}(K_3 + K_4)$, the differences of resistance at the ice point are only about 0.4 of a box unit, but the ratios are based on a considerable number of harmonious observations, and the smallness in their fluctuations from one year to another appears incompatible with any serious error.

The only case in Table XX in which there is unmistakable change in the values of R/R_0 and R_s/R_i with the time is $K_1 - \frac{1}{2}(K_3 + K_4)$. The change in this case is conspicuous, and supports, of course, our previous conclusion that K_1 has undergone some alteration.

§ 49 If we omit combinations containing K_1 or K we notice a curious resemblance between the values of the ratios in the different cases, and this same similarity appears in other combinations of the five thermometers K_3, K_4, K, K, and K (when multiplied by 0.4) which I have tried. We always get something pretty close to —

Ice point difference : steam point difference : sulphur point difference :: 1 : 1.6 : 3.6

The phenomena presented by these five thermometers are thus pretty much the same as if their spirals consisted in the main of given equal lengths of identical platinum with, in addition, variable lengths of a material whose temperature coefficient is higher than that of platinum. In this imaginary material the resistances at the ice, steam and sulphur points would be approximately as 1 : 1.6 : 3.6. Another way of stating the fact would be that the changes in the resistance of the imaginary material due to temperature are about 1.56 times as great as in platinum.

In addition we can closely reproduce the figures resulting from the comparison of K_2 with the other thermometers by supposing that K has a smaller quantity of this imaginary foreign material than the others, but a larger quantity of pure platinum.

The figures to which my calculations lead for the resistances at 0°, due to the foreign material and the pure platinum, are as follows, in terms of the mean box unit —

Thermometer	Resistance foreign material	Resistance platinum wire
K_6	z	$x + 5.150$
K_7	$z + 0.637$	x
K_4	$z + 1.391$	x
K_3	$z + 1.783$	x
K_5	$z + 2.223$	x

Taking, for instance, the cases $K_2 - K_5$ and $K - K_7$ in 1899, we have, in mean box units—

	Observed differences.			Calculated differences.		
	Ice point.	Steam point.	Sulphur point	Ice point.	Steam point.	Sulphur point
$K_3 - K_5$ ..	2·927	3·604	5·564	(2·927)	3·586	5·547
$K_1 - K_7$.	4·518	6·124	11·306	(4·518)	(6·124)	11·257

The numbers enclosed by brackets were used in my original calculation, so their coincidence with the observed values is without significance. The coincidences could have been improved by supposing that the amounts of pure platinum in K_3, K_1, K_5, and K_7 are only nearly and not absolutely the same, and by slightly altering the values 16/10 and 36/10 assumed for the ratios borne by the resistances of the foreign material at the steam and sulphur points to the resistance at the ice point.

That the above coincidences are purely accidental seems improbable, but what the real nature of the physical fact underlying the figures is, I do not know.

Accuracy of the Observations.

§ 50. It is difficult to say what is an equitable basis on which to discuss the question. Presumably if the room containing all the apparatus were kept at a constant temperature, and a uniform source of current were employed; and if arrangements were made for taking the bridge centre, for reversing the battery, and for interchanging the leads and proportional arms at every observation, a variety of sources of uncertainty affecting the Kew results would be much reduced. To provide, however, for all possible interchanges must add to the cost of platinum thermometry, and tend to make observations more tedious. I am thus disposed to think that, except in the case of the most refined physical work, where apparatus is abundant and time and expense of relatively small account, the conditions are unlikely to be more favourable, and will in general be less favourable, than those prevailing during the experiments at Kew Observatory.

Under any circumstances a great deal may depend on whether it is, or is not, deemed essential to make a fixed point observation—either in ice or steam—immediately before or after each series of observations. If a fixed point observation be not taken, we may presumably expect uncertainties in the calculated temperatures of the same order as the fluctuations observed at Kew in R_0 or R_1; while if a fixed point observation is taken, we may presumably expect, when measuring a steady temperature between 0° and 100° C., a similar degree of accuracy to that met with in the determinations of fundamental intervals in the present inquiry.

An idea of the degree of accuracy in the two cases is afforded by the two following tables. The first gives the observed values of R_0 on the days of regular observation since Calibration IV was made; the second gives the corresponding values found for the fundamental intervals. In the first table I give separately the values found when an allowance for the observed difference between the leads is omitted and applied.

Table XXI.—Observed Values of R_0.

Date.	Leads not interchanged.						
1899	K_1	K_2	K_3	K_4.	K_5	K_6	K_7.
	257 +	261 +	257 +	257 +	258 +	645 +	256 +
May 10	·761	·253	·874	·495	·295	·260	·715
June 27, 30 .	·693	·198	·833	·435	·274	·168	·677
July 10 ..	706	—	—	—	280	—	—
Sep 14, 15 ..	·672	·184	·814	·419	·276	·132	·674
Sep 18, 19, 20	·693	198	·845	·445	·286	—	·699

Date.	Leads interchanged.						
1899	K_1.	K_2.	K_3	K_4	K_5.	K_6	K_7.
	257 +	261 +	257 +	257 +	258 +	645 +	256 +
May 10	·768	260	·881	·502	302	·267	·722
June 27, 30 ..	·709	·214	·849	·451	—	·184	—
July 10 ...	·731	—	—	—	·310	—	—
Sep 14, 15 ..	·691	·202	·832	·436	·295	·146	·695
Sep 18, 19, 20	·715	·220	86	·460	303	—	·716

Table XXII.—Observed Values of th Fundamental Interval (leads not interchanged).

Date.	K_1.	K_2.	K_3.	K_4.	K_5.	K_6.	K_7.
1899.	99 +	99 +	99 +	99 +	100 +	250 +	99 +
May 10, 11	·712	·863	·936	·652	·197	·208	·262
June 27, 30	·710	·860	·921	·660	·192	·148	·258
July 10....	·719	—	—	—	·186	—	—
Sept. 14, 15	·701	·849	·918	·655	·171	·162	·254
Sept. 20....	—	—	—	—	·196	—	—

The data in Tables XXI and XXII are a very fair sample of the Kew results. They were taken during a time when the difference between the leads did not vary much, so that the application of a correction for this difference does not much improve the agreement between the results of different days.

The value of R_0 is conspicuously high on May 10 in all the thermometers. This was clearly not the fault of the ice, because, as Table XXII shows, the fundamental intervals found on May 10 were not below the average, but in fact rather above. Again, the cause can hardly have been error in the correction for temperature. For on May 10 the temperature was only very slightly above 20° C., while on the next occasion, June 27 – when the values of R_0 were smaller—it was nearly 22° C., and if we may trust Dr. Harker's experiments, the temperature coefficient applied in the reductions is certainly not too low.

Phenomena similar to those occurring on May 10 were by no means uncommon.

§ 51. Additional information as to the degree of accuracy attained in determining the fundamental interval is afforded by Table XXIII. This gives the mean difference, irrespective of sign, between the individual results and their arithmetic mean for two epochs, viz., the year 1896, and the period from September, 1897, to January, 1899. In the former epoch there were six observations with each thermometer, in the latter epoch from twelve to fourteen.

Table XXIII.—Mean Departures of Fundamental Intervals from their Arithmetic Means.

Epoch.	K_1.	K_3.	K_4.	K_5.	$0{\cdot}4K_6$.	K_7
1896	$\overset{\circ}{0}\cdot015$	$\overset{\circ}{0}\cdot005$	$\overset{\circ}{0}\cdot006$	$\overset{\circ}{0}\cdot007$	$\overset{\circ}{0}\cdot003$	° —
September, 1897, to January, 1899	0·013	0·006	0·010	—	0·008	0·015

During the former epoch the original box with brass plug holes was in use, while during the latter epoch the plug holes were of fusible metal in Doulton ware. During the second epoch bridge centres were taken every day, and readings were always taken with the current both ways; in the first epoch neither of these precautions was taken. Yet we see that the mean departures are almost all greater during the second epoch than during the first. This certainly does not point to any superiority in fusible metal as compared to brass, but quite the reverse.

The size of the mean departures in K_1 is partly due to change in the fundamental interval, especially during the second epoch.

The fundamental interval in K_6, being 2·5 times as large as in the other thermometers, has to be multiplied by 0 4 to render it comparable as an interval of temperature

§ 52 Particulars of the degree of accuracy attainable in determining pt_s may also be of interest In this case, of course the uncertainty existing as to the true law of variation of the boiling point of sulphur with pressure should be borne in mind Taking all the observations made with K K_3, K_4 K and K excluding one with K_4 and the first three with K as probably faulty and employing the Callendar Griffiths formula (10) I find for the average departure of individual values of pt_s from their arithmetic mean the following results —

Table XXIV —Average Departure of Values of pt_s from their Arithmetic Mean

	K	K_3	K_4	K	K_7
Number of observations	6	24	21	10	12
Average departure	±0 04	±0 0[illegible]	±0 07	±0 08	±0 07

The substitution of formula (17) for the Callendar Griffiths formula reduces the average departures in the case of K_3 K_4 and K to ±0 07 ±0 0[illegible] and ±0 05 respectively but increases the mean departures in K_2 and K

Roughly speaking, the average departure from the mean in the case of pt_s is nearly ten times as large as in the case of the fundamental interval This, however may partly arise from the fact that sulphur point observations were frequently taken on the day subsequent to that on which was determined the value of R_0 used in calculating pt_s

Improvements suggested in Apparatus

§ 53 The experiments have suggested various desiderata, which it may be well to summarise

1 It is desirable that some simple and more perfect method should be devised for eliminating the effects of changes in the relative resistance of the leads and the proportional arms

2 The temperature of the resistance coils and bridge wire should be exposed to much less uncertainty than in the Kew box or else these resistances should be made of some material with a much smaller temperature coefficient than platinum silver

3 The bridge wire scale is too much contracted for very exact work with thermometers of such low resistance as the ordinary Kew ones, it is desirable, if accuracy to 0° 001 C is aimed at, that 1 cm of bridge wire should not answer to more than 0° 1 C

4 If possible, the causes of wanderings in the bridge centre and of thermo electric currents—not eliminated by the Griffiths key—should be removed

5 Unless the plug resistance uncertainties can be reduced it would seem desirable to increase the resistance inside platinum thermometers intended for very exact measurement of very slowly altering temperatures such as occur in fixed point observations

6 Unless the heating effect of the current can be exactly ascertained and allowed for the sensitiveness of the galvanometer should be largely increased so as to render unnecessary currents whose heating effects will sensibly influence the readings

7 It is certainly desirable that the entire room containing the apparatus should be kept approximately at the temperature accepted as the standard temperature for the coils At the same time it is not desirable that the observer should be continually exposed to so high a temperature as 20 C A temperature such as 62 F is very much healthier at least in winter and is more bracing There is no obvious reason why there might not be one standard temperature for winter and another for summer so long as the temperature relations of the resistance coils are accurately known

Further Experiments Wanted

§ 54 I should like to see experiments made on the following points —

1 The true law of the variation of the boiling point of sulphur with pressure

2 The behaviour of *new* platinum thermometers under a variety of conditions

3 The effect of long or frequent exposures to temperatures extending from that of sulphur vapour to the highest temperature for which it is claimed that platinum thermometers are suitable

4 The effect of exposure to low temperatures such as are now attainable by means of liquid air,

5 The degree of accordance in the results deduced by Callendar's formula from observations taken with different platinum thermometers—varying in their δ—at two or three fixed temperatures differing considerably from that of sulphur vapour, those temperatures being obtainable with a degree of accuracy not inferior to that attained in sulphur point observations,

6 The cause of the curious phenomenon described in §§ 48 and 49

§ 55 The great majority of the observations on which this paper depends have been made by Mr. W. Hugo, senior assistant at Kew Observatory, to whose care and exactitude the research owes a very great deal. The reduction of the observations is also mainly due to Mr. Hugo. In several of the special experiments Dr. J. A. Harker has given valuable assistance, and various operations have been much facilitated by means of apparatus devised by him. The main work of checking the reductions and the various calculations requisite in discussing the observations have been done by myself. I have also taken a considerable number of observations, and in discussing the various sources of trouble I speak from personal experience, supplemented of course by frequent exchange of ideas with Dr. Harker and Mr. Hugo. At various stages of the investigation I have had valuable advice and assistance from Professor Carey Foster and Mr. W. N. Shaw, to whom the initiation of the research is largely due.

The original cost of the apparatus was partly defrayed by a grant of £100 from the Government Grant Fund, and a contribution of £120 was received last year from the Gunning Fund towards this and other cognate work undertaken by the Kew Observatory Committee.

A Comparative Crystallographical Study of the Double Selenates of the Series $R_2M(SeO_4)_2.6H_2O$—Salts in which M is Zinc.

By A. E. TUTTON, B.Sc., F.R.S. Received March 5,—Read March 15, 1900.

(Extended Abstract.)

In two communications to the Chemical Society,* the author presented the results of a detailed study of twenty-two salts of the series of monoclinic double sulphates $R_2M(SO_4)_2.6H_2O$, in which R was represented by potassium, rubidium, and cæsium, and M by magnesium, zinc, iron, manganese, nickel, cobalt, copper, and cadmium. The first of these memoirs dealt with the external morphology of the crystals, and the second with their internal physical properties.

The present investigation refers to the less known analogous double selenates, in which R is again represented by the alkali metals potassium (atomic weight 39), rubidium (atomic weight 85.2), and cæsium (atomic weight 132.7). The work on the group containing zinc has been completed, and the results are now communicated. Topsoe and Christiansen† included in their well known investigation the potassium salt of the group.

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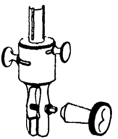
* 'Journ. Chem. Soc. Trans.,' 1893, vol. 63, p. 337, and 1896, vol. 69, p. 344.

† 'Ann. de Chim. et de Phys.,' 1874, p. 5.

The rubidium and cæsium salts have never been investigated, and the author has been unable to find any evidence of their preparation.

The section plates and prisms employed in the optical work were prepared by means of the author's cutting and grinding goniometer, in the manner already described in previous memoirs. The new special adjusting apparatus described to the Royal Society in the memoir in the Phil. Trans., A, for 1899, p. 461, has proved of the greatest service in the preparation of the prisms. All the prisms were obtained with its aid by one adjustment instead of two (one for each of the two required surfaces). A very useful addition to it has been made in the shape of a pair of special grip holders besides those referred to on page 460 of the memoir just quoted. One of these new holders is represented in fig. 1. The upper portion, the stem, by which it is attached to the lower end of the crystal adjusting apparatus carries two vertical grooves instead of only one, so that the plane within the crystal which is desired to bisect the angle (60°) of the prism can always be preliminarily roughly arranged at right angles to the 120° adjusting segment, whatever be the situation of the most convenient direction of gripping. The stem passes down into a thicker solid cylindrical portion surrounded by a closely fitting hollow cylinder capable of movement for somewhat over 90° and fixation at any position by means of slits at different levels and two clamping screws passing through them into the solid cylinder. This enables the necessary final azimuth adjustment of the crystal to be effected, so that the particular bisecting plane referred to can be set exactly to the required orientation with the aid of the goniometer and its graduated adjusting movements. The gripper is attached rigidly to the underside of the movable cylinder. Its two prongs are arranged to be drawn together by means of a screw manipulated with a milled headed key, and the gripping lower part is thickened and padded with chamois leather. The use of one of these holders which only differ in the size of the gripper so as to accommodate different sized crystals, avoids the use of warm wax, which may cause efflorence, or cracking, of the crystal.

FIG. 1.



Every prism employed was prepared by the aid of this instrument so that the two surfaces were symmetrical to one of the three principal planes of the optical ellipsoid (indicatrix), and its edge parallel to one of the two rectangular axes of the indicatrix lying in this plane. It therefore yielded two of the three refractive indices directly, namely, those corresponding to vibrations parallel to the two rectangular axes of the optical indicatrix lying in the bisecting plane.

The salts were prepared in the following manner:—A quantity of the alkaline selenate crystals, prepared as described in the author's memoir on the normal alkaline selenates,* adequate to yield after addition to the calculated amount of the M selenate sufficient of the double salt for all the purposes of the investigation was dissolved in the minimum of distilled water An equal molecular proportion of pure zinc sulphate crystals was also weighed out dissolved in distilled water, and precipitated by a solution of sodium carbonate The precipitate of basic zinc carbonate was isolated by decantation and prolonged washing with hot water and subsequently dissolved in a solution of pure selenic acid, whose strength had previously been accurately determined by titration with a solution of pure anhydrous sodium carbonate of known strength Sufficient of the acid was run in upon the zinc carbonate to provide for one drop of excess, as this prevents the possibility of a basic salt forming after addition of the selenate of the R-metal The solutions of the two selenates were mixed and allowed to crystallise spontaneously taking all the precautions to avoid disturbance enumerated in the previous memoirs

The spherical projection given on p 343 of the double sulphate memoir applies equally to the double selenates

POTASSIUM ZINC SELENATE $K_2Zn(SeO_4)_2 6H_2O$

A determination of the zinc in a quantity of the crystals employed gave the following result —0 9152 gramme yielded 0 1396 gramme ZnO, corresponding to 12 24 per cent of zinc Calculated per cent Zn 12 11

Goniometry

Ten excellent crystals were employed derived from several different crops

Habit Short prismatic to tabular

Axial angle— $\beta = 75\ 48$

Ratio of axes—

$$a : b : c = 0\ 7458 : 1 : 0\ 5073$$

Forms observed—

$$a = \{100\}\infty P\infty,\ b = \{010\}\infty \grave{R}\infty,\ c = \{001\}oP,\ p = \{110\}\infty P,$$

$$p = \{120\}\infty \grave{R}2,\ q = \{011\}\grave{R}\infty,\ o = \{\bar{1}11\}+P,\ r = \{\bar{2}01\}+2P\infty$$

The results of the angular measurements are given in the accompanying table

The habits observed resembled those exhibited in the following figures given in the double sulphate memoir —Fig 7 without *b* faces, fig 8 without the smaller faces, fig 20, fig 21, fig 24, and fig 25

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* Journ Chem Soc,' 1897, p 846

Morphological Angles of Potassium Zinc Selenate

Angle measured	No. of measurements	Limits	Mean observed	Calculated	Diff.
		° °	°	°	
ac = 100 001 = β	5	75 43—75 55	75 45	75 48	3
as = 100 101		—	—	46 19	—
sc = 101 001	—		—	20 29	—
cr = 001 $\bar{2}01$	16	63 4—63 25	63 15	63 12	3
cs = 001 $\bar{1}01$	—	—	—	38 22	—
sr = $\bar{1}01$ 201	—	—	—	21 50	—
ra = $\bar{2}01$ $\bar{1}00$	5	40 50—41 0	40 56	41 0	4
rc = $\bar{2}01$ $00\bar{1}$	16	116 35—116 56	116 15	116 18	3
ap = 100 110	8	35 33—36 8	35 52	*	—
pp = 110 120	2	19 1—19 45	19 23	19 28	5
pb = 120 010	1		34 39	34 40	1
pb = 110 010	10	53 51—54 24	54 7	54 8	1
pp = 110 $1\bar{1}0$	20	71 31—71 54	71 43	71 44	1
pp = 110 $\bar{1}10$	20	108 0—108 32	108 16	108 16	0
cq = 001 011	36	26 0—26 23	26 8	*	—
qb = 011 010	12	63 35—64 8	63 52	63 52	0
qq = 011 $01\bar{1}$	13	127 38—127 53	127 44	127 44	0
ao = 100 111	—	—	—	41 35	—
oq = 111 011	—	—	—	27 42	—
aq = 100 011	—	—	—	7 17	—
qo = 011 $\bar{1}11$	—	—	—	34 41	
oa = $\bar{1}11$ $\bar{1}00$	—	—	—	68 12	—
co = 001 111	—	—	—	35 11	—
op = 111 110	—	—		43 21	—
cp = 001 110	42	78 22—78 41	78 32	*	—
po = 110 $11\bar{1}$	3	56 42—56 59	56 48	56 50	2
oc = $11\bar{1}$ $00\bar{1}$	3	44 37—44 42	44 39	44 38	1
pc = 110 $00\bar{1}$	42	101 21—101 38	101 29	101 28	1
bo = 010 111	—	—	—	69 51	—
os = 111 101	—	—	—	20 9	—
bo = 010 $\bar{1}11$	—	—		65 10	—
os = $\bar{1}11$ $\bar{1}01$	—	—	—	21 50	—
sq = 101 011	—	—	—	38 36	—
qp = 011 $\bar{1}10$	35	85 14—85 48	85 29	85 26	3
ps = $\bar{1}10$ $\bar{1}0\bar{1}$	—	—	—	55 58	—
pq = $\bar{1}10$ $0\bar{1}\bar{1}$	35	94 14—94 49	94 31	94 34	3
s′q = $\bar{1}01$ 011	—	—	—	45 15	—
qp = 011 110	34	63 59—64 24	64 9	64 7	2
ps′ = 110 $10\bar{1}$	—	—	—	70 38	—
pq = 110 $0\bar{1}\bar{1}$	34	115 27—116 0	115 51	115 53	2
r′o′ = $\bar{2}01$ $\bar{1}11$	3	34 27—34 46	34 33	34 33	0
o′p = $\bar{1}11$ 110	3	93 10—93 26	93 17	93 10	7
pr′ = 110 $20\bar{1}$	31	52 1—52 28	52 17	52 17	0
r′p = $\bar{2}01$ 110	31	127 20—127 58	127 43	127 43	0

Total number of measurements, 462

The *a* and *b* faces were usually both well defined, the hemi pyramid *o* was occasionally well developed, but more usually small, while small *p* faces were only observed on two crystals The general type exhibited *p*, *c*, *r* and *q* as the main faces *c* always large, and *r* occasionally large but generally rather smaller than the *q* faces

Topsoe and Christiansen (*loc cit*, p 77) give for the ratio of the axes and the axial angle, $a : b : c = 0.7441 : 1 : 0.5075$, and $\beta = 75° 46'$ The measurements were made by Topsoe in the year 1870

There is an excellent cleavage parallel to the faces of *r* {201}, as stated by Topsoe and Christiansen

Volume

Relative Density—The following four independent determinations were made —

Weight of salt employed	Sp gr at 20°/4°
5.5358	2.5535
6.1058	2.5544
5.1597	2.5524
5.0657	2.5546
Mean	2.5537

Molecular Volume—$\dfrac{M}{d} = \dfrac{536.6}{2.5537} = 210.13$

Distance Ratios—Combination of the axial ratios and axial angle previously given, with the molecular volume, affords the following distance ratios —

$$\chi : \psi : \omega = 6.1941 : 8.3054 : 4.2133$$

Optics

Orientation of Axes of Optical Ellipsoid—The plane of the optic axes (optic binormals) is the plane of symmetry The sign of the double refraction is positive

Two section plates ground parallel to the symmetry plane afforded the following extinction angles, relative to the normal to the basal plane —

Section 1	5° 30'
„ 2	4 36
Mean	5° 3'

The direction is behind the normal, nearer to the vertical axis

This axis of the optical ellipsoid (indicatrix) is the second median line. The first median line is consequently situated in the obtuse angle of the morphological axes *ac*, and is inclined 5° 3′ to the axis *a*. The second median line lies also in the obtuse angle *ac*, and is inclined 9° 9′ to the vertical axis *c*.

Refractive Indices.—The results of the determinations with six prisms, ground on six different crystals, are given in the accompanying table. The values obtained by Topsoe and Christiansen are appended in the last column. The β values, which were alone determined directly by them, are observed to agree well with the authors' values.

The intermediate refractive index of potassium zinc selenate, corrected to a vacuum, is accurately expressed as far as the neighbourhood of F, for any wave-length λ, by the following formula:—

$$\beta = 1{\cdot}5010 + \frac{694\,100}{\lambda^2} - \frac{3\,005\,700\,000\,000}{\lambda^4} + \ldots$$

As the dispersion increases with the numerical value of the index, the α and γ indices are not precisely reproduced by diminishing and increasing the constant 1·5010 by fixed amounts; but they are on the average less and greater respectively than the β values by 0·0060 and 0·0154.

Alteration of Refraction by Rise of Temperature.—Determinations carried out at 60° indicated that the indices are diminished by about 0·0020 for 45° rise of temperature.

Axes of the Optical Ellipsoid.—The calculated values of the axial ratios of the two optical ellipsoids are as follows:—

Axes of optical indicatrix:

$$\alpha : \beta : \gamma = 0{\cdot}9960 : 1 : 1{\cdot}0101.$$

Axes of optical velocity ellipsoid:

$$\mathfrak{a} : \mathfrak{b} : \mathfrak{c} = 1{\cdot}0040 : 1 : 0{\cdot}9900.$$

Refractive Indices of Potassium Zinc Selenate

	Light	Prism 1	Prism 2	Prism 3	Prism 4	Prism 5	Prism 6	Mean	Values of Topsoe and Christiansen
α Vibrations parallel 2nd median line	Li	1 5094	1 5092	1 5082	—	1 5080	—	1 5087	—
	C	1 5099	1 5099	1 5086	—	1 5085	—	1 5092	—
	Na	1 5128	1 5125	1 5115	—	1 5114	—	1 5121	1 5115
	Tl	1 5158	1 5155	1 5146	—	1 5144	—	1 5151	—
	F	1 5195	1 5194	1 5184	—	1 5181	—	1 5189	—
	G	1 5250	1 5249	1 5238	—	1 5237	—	1 5244	—
β Vibrations parallel symmetry axis *b*	Li	1 5157	—	1 5145	1 5143	—	1 5140	1 5146	—
	C	1 5162	—	1 5151	1 5145	—	1 5145	1 5151	1 5148
	Na	1 5193	—	1 5179	1 5177	—	1 5176	1 5181	1 5177
	Tl	1 5223	—	1 5210	1 5206	—	1 5205	1 5212	—
	F	1 5263	—	1 5251	1 5248	—	1 5247	1 5252	1 5258
	G	1 5317	—	1 5305	1 5305	—	1 5302	1 5307	1 5308
γ Vibrations parallel 1st median line	Li	—	1 5305	—	1 5294	1 5295	1 5295	1 5297	—
	C	—	1 5309	—	1 5300	1 5300	1 5300	1 5302	—
	Na	—	1 5343	—	1 5333	1 5332	1 5333	1 5335	1 5327
	Tl	—	1 5377	—	1 5365	1 5367	1 5365	1 5369	—
	F	—	1 5417	—	1 5407	1 5407	1 5408	1 5410	—
	G	—	1 5478	—	1 5469	1 5469	1 5469	1 5471	—

Molecular Optical Constants.—Following are the values of these constants:—

Axis of optical indicatrix.		α.	β.	γ.
Specific refraction, $\frac{\mu^2-1}{(\mu^2+2)d} = n$	C	0·1170	0·1181	0·1210
	G	0·1199	0·1211	0·1242
Molecular refraction, $\frac{\mu^2-1}{\mu^2+2}\frac{M}{d} = m$..	C	62·76	63·37	64·93
	G	64·38	64·98	66·65
Specific dispersion, $n_G - n_C$		0·0029	0·0030	0·0032
Molecular dispersion, $m_G - m_C$		1·57	1·61	1·72
Molecular refraction, $\frac{\mu-1}{d}M$	C	107·00	108·24	111·41

Optic Axial Angle.—Three excellent pairs of section-plates were ground, perpendicular to the first and second median lines respectively.

Determination of Apparent Angle in Air of Potassium Zinc Selenate.

Light.	Section 1.	Section 2.	Section 3.	Mean 2E.
Li	112° 12′	111° 34′	111° 56′	111° 54′
C	112 14	111 40	112 0	111 58
Na	112 24	112 8	112 19	112 17
Tl	112 40	112 35	112 41	112 39
F	112 58	113 1	113 10	113 3

Topsoe and Christiansen give for the angle in air 111° 50′, and for the true angle 66° 8′, both being measured in sodium light.

The dispersion is observed to be extremely small, and the most accurate measurement is required to determine it, employing sections which afford very small rings and sharp brushes. A valuable confirmation of the nature of the dispersion is afforded by immersion in cedar oil, whose refraction is almost exactly the same as that of the crystals. A section perpendicular to the first median line shows, in cedar oil, brushes separated at their true angle and fringed with colour according to the following scheme:—

red | blue   1st M.L.   no colour | no colour.

The obtuse morphological axial angle *ac* is assumed to be to the left. The optic axial angle is therefore greater for blue than for red, and measurements in Li and F light gave an angle larger in the latter case by about 5′, thus confirming the accuracy of the values given in the table.

Determination of True Optic Axial Angle of Potassium Zinc Selenate.

Light.	No. of section perpendicular 1st median line	Observed values of 2Ha.	No of section perpendicular 2nd median line.	Observed values of 2Ho.	Calculated values of 2Va	Mean value of 2Va.
		° ′		° ′	° ′	° ′
Li	1	60 7	1*a*	100 12	66 16	66 12
	2	59 54	2*a*	100 8	66 8	
	3	60 3	3*a*	100 14	66 13	
C	1	60 5	1*a*	100 8	66 16	66 13
	2	59 52	2*a*	100 4	66 8	
	3	60 0	3*a*	100 6	66 14	
Na	1	59 54	1*a*	99 42	66 18	66 15
	2	59 43	2*a*	99 39	66 10	
	3	59 49	3*a*	99 39	66 16	
Tl	1	59 39	1*a*	99 5	66 20	66 17
	2	59 31	2*a*	99 3	66 14	
	3	59 36	3*a*	99 5	66 18	
F	1	59 17	1*a*	98 17	66 22	66 20
	2	59 11	2*a*	98 15	66 18	
	3	59 19	3*a*	98 25	66 20	

Dispersion of the Median Lines.—In cedar oil no appreciable movement of the right brush, that corresponding to the optic axis situated in the acute morphological angle *ac*, was detected on altering the wave-length of the light, the total movement occurring at the left brush. Hence the first median line lies nearer to the morphological axis *a* for red than for blue. The amount does not exceed 5′.

Effect of Rise of Temperature on the Optic Axial Angle.—Measurements at 65° indicated that 2E increases $2\frac{1}{2}$° for 50° rise of temperature.

RUBIDIUM ZINC SELENATE, $Rb_2Zn(SeO_4)_2,6H_2O$.

An estimation of zinc in a specimen of the crystals employed afforded the following guarantee of purity: 1·1130 grammes yielded 0·1426 gramme ZnO, which corresponds to 10·28 per cent. of zinc. Calculated Zn = 10·33.

Goniometry.

Ten suitable small crystals were employed, belonging to four different crops.

Habit: thick tabular, sometimes prismatic.

Morphological Angles of Rubidium Zinc Selenate

Angle measured	No. of measurements	Limits	Mean observed	Calculated	Diff.
		° °	°		
$ac = 100\ 001\ \beta$	—		—	74 44	—
$as = 100\ 101$	—	—	—	45 47	—
$sc = 101\ 001$	—	—	—	28 7	—
$r = 001\ \bar{2}01$	9	63 45— 64 [illegible]	63 53	63 42	11
$cs = 001\ \bar{1}01$	—		—	38 24	—
$sr = \bar{1}01\ \bar{2}01$	—	—	—	25 18	—
$ra = \bar{2}01\ \bar{1}00$		—	—	41 34	—
$r = \bar{2}01\ 00\bar{1}$	11	115 57—116 15	116 4	116 18	14
$ap = 100\ 110$	—	—	—	35 38	—
$pp' = 110\ 120$	—	—	—	19 28	—
$p'l = 120\ 010$	—		—	34 54	—
$pl = 110\ 010$	2	54 15— 54 27	54 21	54 22	1
$pp = 110\ 1\bar{1}0$	20	71 6— 71 39	71 15	*	—
$pp = 110\ \bar{1}10$	20	108 21 109 1	108 45	108 45	0
$cq = 001\ 011$	21	25 35— 26 7	25 50	*	—
$qb = 011\ 010$	[illegible]	64 10— 64 16	64 13	64 10	3
$qq = 011\ 01\bar{1}$	17	128 8—128 26	128 25	128 20	5
$ao = 100\ 111$	—	—	—	48 55	—
$oq = 111\ 011$	—	—		27 1[illegible]	—
$aq = 100\ 011$		—		76 18	—
$qo = 011\ \bar{1}11$	—	—		34 37	—
$oa = \bar{1}11\ \bar{1}00$	—		—	69 5	
$co = 001\ 111$		—	—	34 34	—
$op = 111\ 110$	—	—		43 5	—
$cp = 001\ 110$	35	77 28— 77 57	77 39	*	—
$po = 110\ 11\bar{1}$	3	57 33— 57 37	57 35	57 43	8
$oc = 11\bar{1}\ 00\bar{1}$	3	44 40— 44 47	44 45	44 38	7
$pc = 110\ 00\bar{1}$	34	102 10—102 36	102 21	102 21	0
$bo = 010\ 111$	—	—		70 13	—
$os = 111\ 101$	—	—	—	19 47	—
$bo = 010\ \bar{1}11$	8	65 9— 65 17	65 14	65 14	0
$os = \bar{1}11\ \bar{1}01$		—	—	24 46	—
$o = \bar{1}11\ \bar{1}\bar{1}1$	1	—	49 34	49 32	2
$sq = 101\ 011$	—		—	38 2	—
$qn = 011\ \bar{1}21$	1		35 46	35 33	13
$np = \bar{1}21\ \bar{1}10$	1	—	50 44	50 57	13
$qp = 011\ \bar{1}10$	26	86 14— 86 49	86 37	86 30	7
$ps = \bar{1}10\ \bar{1}0\bar{1}$	—	—	—	55 28	—
$pq = \bar{1}10\ 0\bar{1}\bar{1}$	26	93 6 93 36	93 23	93 30	7
$sq = \bar{1}01\ 011$	—		—	45 8	—
$qp = 011\ 110$	28	63 26— 63 55	63 38	63 30	8
$ps = 110\ 10\bar{1}$	—	—	—	71 22	—
$pq = 110\ 0\bar{1}\bar{1}$	28	116 6 116 38	116 23	116 30	7
$r'o = \bar{2}01\ \bar{1}11$	3	34 51— 34 53	34 52	34 49	3
$op = \bar{1}11\ 110$	3	92 44— 92 47	92 45	92 39	6
$pr = 110\ 20\bar{1}$	29	52 12— 52 32	52 21	52 32	11
$r'p = \bar{2}01\ 110$	28	127 23—127 50	127 39	127 28	11

Total number of measurements, 354.

Axial angle $\beta = 74° 44'$.
Ratio of axes $a : b : c = 0.7431 : 1 : 0.5019$.
Forms observed $a = \{100\}\infty\bar{P}\infty$, $b = \{010\}\infty\breve{P}\infty$, $c = \{001\}0P$,
$p = \{110\}\infty P$, $q = \{011\}\breve{P}\infty$, $o = \{111\}+P$,
$r = \{\bar{2}01\}+2\bar{P}\infty$, $n = \{121\}+2\breve{P}2$.

The results of the measurements are tabulated in the accompanying table.

The types observed resembled those of rubidium zinc sulphate, especially that shown in fig. 7 (p. 359) of the double sulphate memoir, but without the q faces as a rule. Only a trace of the orthopinacoid a was discovered; the clinopinacoid b was invariably small when present, and the hemi-pyramid n was only present on one of the crystals measured. The faces were fairly well developed, and afforded good images of the signal. Frequently the only faces present were c, p, r, and q. The c faces varied in relative importance from the breadth usually exhibited in the potassium salts to the narrow strip characteristic of the cæsium salt.

There is an excellent cleavage parallel to the faces of the orthodome $r\{\bar{2}01\}$.

Volume.

Relative Density.—The following four determinations were made with independent quantities of the finely powdered crystals:—

Weight of salt employed.		Sp. gr. at 20°/4°.
7.8234		2.8596
6.0506		2.8611
7.8824		2.8601
6.7490		2.8608
	Mean	2.8604

Molecular Volume.—$\frac{M}{d} = \frac{629.0}{2.8604} = 219.90$.

Distance Ratios.—On combining the axial angle and the ratio of the axes already given, with the molecular volume, the following ratios are obtained:—

$$\chi : \psi : \omega = 6.3062 : 8.4863 : 4.2593.$$

Optics.

Orientation of Axes of Optical Ellipsoid.—The optic axes (optic binormals) lie in the plane of symmetry. The sign of the double refraction is positive.

The following extinction angles were exhibited by two section-plates ground parallel to the symmetry plane, with reference to the normal to the basal plane :—

Section 1	2°	0′
Section 2	2	7
Mean .. .	2	3

The direction is behind the normal, towards the vertical morphological axis This axis of the optical indicatrix is again the second median line.

The first median line thus lies in the obtuse morphological axial angle *ac*, and is inclined 2° 3′ to the axis *a*. The second median line lies also in the obtuse angle *ac*, and is inclined 13° 13′ to the vertical axis *c*.

Refractive Indices.—Six prisms, ground on six different crystals belonging to various crops, were employed in the determinations. The results are given in the accompanying table :—

Refractive Indices of Rubidium Zinc Selenate.

Index	Light	Prism 1	Prism 2	Prism 3	Prism 4	Prism 5	Prism 6	Mean
α Vibr. par. 2 M.L.	Li	1·5123	—	—	1·5132	1·5135	1·5126	1·5129
	C	1·5128	—	—	1·5136	1·5141	1·5132	1·5134
	Na	1·5157	—	—	1·5165	1·5167	1·5159	1·5162
	Tl	1·5188	—	—	1·5197	1·5201	1·5190	1·5194
	F	1·5228	—	—	1·5234	1·5240	1·5229	1·5233
	G	1·5284	—	—	1·5288	1·5295	1·5284	1·5288
β Vibr. par. symm. axis.	Li	1·5187	1·5175	1·5198	—	1·5190	—	1·5188
	C	1·5192	1·5181	1·5201	—	1·5197	—	1·5193
	Na	1·5220	1·5210	1·5230	—	1·5227	—	1·5222
	Tl	1·5251	1·5241	1·5261	—	1·5258	—	1·5253
	F	1·5291	1·5280	1·5302	—	1·5297	—	1·5293
	G	1·5350	1·5338	1·5360	—	1·5354	—	1·5351
γ Vibr. par. 1 M.L.	Li	—	1·5286	1·5299	1·5298	—	1·5294	1·5294
	C	—	1·5290	1·5304	1·5303	—	1·5298	1·5299
	Na	—	1·5323	1·5336	1·5335	—	1·5330	1·5331
	Tl	—	1·5355	1·5369	1·5370	—	1·5367	1·5365
	F	—	1·5396	1·5408	1·5411	—	1·5405	1·5405
	G	—	1·5457	1·5469	1·5472	—	1·5466	1·5466

The intermediate refractive index of rubidium zinc selenate, corrected to a vacuum, is expressed as far as F, for any wave-length λ, by the formula :—

$$\beta = 1{\cdot}5067 + \frac{592\ 314}{\lambda^2} - \frac{1\ 397\ 600\ 000\ 000}{\lambda^4} + \ldots.$$

The α indices are very closely reproduced by the formula if the constant 1·5067 is diminished by 0·0060; owing to appreciably increased dispersion in the γ direction, the γ indices are not so accurately reproduced, but are approximately given if the constant is increased by 0·0109.

Alteration of Refraction by Rise of Temperature.—Determinations at 60° showed that the indices are reduced by 0·0018 by 45° rise of temperature (15—60°).

Axes of the Optical Ellipsoid.—Following are the values of the axial ratios of the two optical ellipsoids—

Axes of optical indicatrix:

$$\alpha : \beta : \gamma = 0·9961 : 1 : 1·0072.$$

Axes of optical velocity ellipsoid:

$$\mathfrak{a} : \mathfrak{b} : \mathfrak{c} = 1·0039 : 1 : 0·9929.$$

Molecular Optical Constants.—The calculated values of these constants are as follows:—

Axis of optical indicatrix.		α.	β.	γ.
Specific refraction, $\frac{n^2-1}{(n^2+2)d} = \mathfrak{n}$......	C	0·1051	0·1062	0·1080
	G	0·1078	0·1089	0·1108
Molecular refraction, $\frac{n^2-1}{n^2+2} \cdot \frac{M}{d} = \mathfrak{m}$	C	66·13	66·77	67·92
	G	67·80	68·47	69·70
Specific dispersion, $\mathfrak{n}_G - \mathfrak{n}_C$		0·0027	0·0027	0·0028
Molecular dispersion, $\mathfrak{m}_G - \mathfrak{m}_C$		1·67	1·70	1·78
Molecular refraction, $\frac{n-1}{d}M$...........	C	112·90	114·20	116·53

Optic Axial Angle.—The following measurements were obtained with the aid of three pairs of section-plates, ground perpendicular to the first and second median lines, according to the indications of their orientation afforded by the extinction angles:—

Apparent Angle in Air of Rubidium Zinc Selenate.

Light	Section 1.		Section 2.		Section 3.		Mean 2E.	
	°	′	°	′	°	′	°	′
Li	135	2	139	0	138	22	137	28
C	135	7	139	6	138	28	137	34
Na	135	23	139	32	139	5	138	0
Tl	135	43	139	55	139	40	138	26
F	136	15	140	25	140	19	139	0

Determination of True Optic Axial Angle of Rubidium Zinc Selenate.

Light.	No of section perpendicular 1st median line	Observed values of 2H*a*.	No of section perpendicular 2nd median line.	Observed values of 2H*o*.	Calculated values of 2V*a*	Mean value of 2V*a*
		° ′		° ′	° ′	° ′
Li	1	67 50	1*a*	93 30	74 57	75 16
	2	68 21	2*a*	93 40	75 12	
	3	69 0	3*a*	93 44	75 38	
C	1	67 55	1*a*	93 35	74 56	75 14
	2	68 17	2*a*	93 38	75 10	
	3	68 56	3*a*	93 40	75 37	
Na	1	67 31	1*a*	93 10	74 50	75 8
	2	67 52	2*a*	93 16	75 2	
	3	68 35	3*a*	93 20	75 32	
Tl	1	67 8	1*a*	92 45	74 45	75 2
	2	67 20	2*a*	92 54	74 56	
	3	68 12	3*a*	92 56	75 26	
F	1	66 37	1*a*	92 10	74 38	74 55
	2	67 1	2*a*	92 26	74 48	
	3	67 45	3*a*	92 32	75 18	

The dispersion is thus seen to be small, but it is distinctly greater than in the case of the potassium salt. In cedar oil, whose refraction is almost exactly the same as that of this salt, a section perpendicular to the first median line exhibits the brushes fringed with colour as follows:—

blue | red   1st M.L.   red | blue

The optic axial angle is therefore greater for red than for blue, thus confirming the results of the measurements.

Dispersion of the Median Lines.—In the foregoing representation of the coloured brush-fringes as seen in cedar oil, the obtuse angle of the morphological axes *ac* is situated to the left. The hyperbolic brush on this side was more faintly tinted at the edges than the right-hand brush, indicating feebler dispersion of the optic axis to the left. Measurements in cedar oil confirmed this, and showed that the difference between the position of this optic axis for C-light and for F-light was 8—10′, whilst for the right-hand axis it was 20′. Hence the median lines are dispersed so that the first median line lies nearer to the morphological axis *a* by 4—5′ for red light than it does for blue.

Effect of Rise of Temperature on the Optic Axial Angle.—Very little change is introduced in the optic axial angle of this salt by variation of temperature. Measurements in succession at 10.5° and 60.5° indicated that 2E increases 30' for 50° of rise of temperature.

CÆSIUM ZINC SELENATE, $Cs_2Zn(SeO_4)_2,6H_2O$

The analysis of a specimen of the crystals of this salt afforded the following numbers: 0.7027 gramme gave 0.0790 gramme of ZnO, which corresponds to 9.02 per cent. of zinc. The calculated percentage is 8.98.

Goniometry.

Eleven of the most suitable small crystals, derived from five different crops, were employed in the goniometrical measurements.

Habit: flattened prismatic.

Axial angle: $\beta = 73° 49'$.

Ratio of axes: $a : b : c = 0.7314 : 1 : 0.4971$.

Forms observed: $b = \{010\}\infty \grave{P} \infty$, $c = \{001\}0P$, $p = \{110\}\infty P$,
$q = \{011\}\grave{P}\infty$, $n = \{021\}2\grave{P}\infty$, $o = \{\bar{1}11\}+P$,
$r = \{\bar{2}01\}+2\bar{P}\infty$.

The results of the measurements are presented in the accompanying table of angles.

The habits of the crystals of the various crops are typified by the following figures of the double sulphate memoir (*loc. cit.*): figs. 9 and 10 in the description of cæsium zinc sulphate, fig. 16 in the description of cæsium ferrous sulphate, fig. 21, but with much larger *q* faces, and fig. 34 in the description of cæsium cadmium sulphate as far as regards the relation of the *c*, *r* and *p* faces. They are characterised by large *q* faces relatively to the faces of the basal plane, which latter is usually only represented by a mere strip. The faces of the hemipyramid *o* are often considerably developed.

The clinopinacoid *b* is frequently present, but the orthopinacoid *a* was never observed. The clinodome *n* was found developed on one of the crystals measured.

The cleavage parallel to $r\{\bar{2}01\}$, common to the series, was well developed.

Morphological Angles of Cæsium Zinc Selenate.

Angle measured	No. of measurements.	Limits.	Mean observed.	Calculated	Diff
		° ′ ° ′	° ′	° ′	′
$ac = 100 . 001 = \beta$	—	—	—	73 49	—
$as = 100 : 101$	—	—	—	45 8	—
$sc = 101 . 001$	—	—	—	28 46	—
$cr' = 001 \cdot \bar{2}01$	22	64 36 – 64 53	64 45	64 36	9
$cs' = 001 : \bar{1}01$	—	—	—	38 52	—
$s'r' = \bar{1}01 \quad \bar{2}01$	—	—	—	25 44	—
$r'a = \bar{2}01 : \bar{1}00$	—	—	—	41 35	—
$r'c = \bar{2}01 \quad 00\bar{1}$	19	115 7 115 32	115 16	115 24	8
$ap = 100 \quad 110$	—	—	—	35 4	—
$pp' = 110 \cdot 120$	—	—	—	19 28	—
$p'b = 120 \cdot 010$	—	—	—	35 28	—
$pb = 110 \cdot 010$	6	54 46 – 55 4	54 54	54 56	2
$pp = 110 \quad 1\bar{1}0$	19	69 55 – 70 21	70 7	*	—
$pp = 110 \quad \bar{1}10$	20	109 25 – 110 16	109 52	109 53	1
$cq = 001 . 011$	30	25 22 – 25 42	25 31	*	—
$qb = 011 \quad 010$	4	64 18 – 64 43	64 33	64 29	4
$qq = 011 \quad 01\bar{1}$	14	128 40 – 129 12	129 0	128 58	2
$ao = 100 : 111$	—	—	—	48 12	—
$oq = 111 . 011$	—	—	—	27 14	—
$aq = 100 . 011$	—	—	—	75 26	—
$qo' = 011 : \bar{1}11$	—	—	—	35 6	—
$o'a = \bar{1}11 : \bar{1}00$	—	—	—	69 28	—
$oo, = 001 : 111$	—	—	—	34 18	—
$op = 111 . 110$	—	—	—	42 36	—
$cp = 001 \cdot 110$	37	76 34 – 76 57	76 49	*	—
$po' = 110 \cdot 11\bar{1}$	16	57 52 – 58 34	58 10	58 14	4
$o'c = 11\bar{1} \cdot 00\bar{1}$	17	44 35 – 45 21	45 0	44 57	3
$pc = 110 : 00\bar{1}$	36	102 59 – 103 39	103 11	103 11	0
$bo = 010 . 111$	—	—	—	70 37	—
$os' = 111 : 101$	—	—	—	19 23	—
$bo' = 010 : \bar{1}11$	—	—	—	65 22	—
$o's' = \bar{1}11 \cdot \bar{1}01$	—	—	—	24 38	—
$sq = 101 : 011$	—	—	—	87 43	—
$qp = 011 \cdot \bar{1}10$	36	87 24 – 88 3	87 39	87 37	2
$ps = \bar{1}10 : \bar{1}0\bar{1}$	—	—	—	54 40	—
$pq = \bar{1}10 . 0\bar{1}\bar{1}$	36	91 52 – 92 36	92 21	92 23	2
$s'q = \bar{1}01 : 011$	—	—	—	45 21	—
$qp = 011 : 110$	35	62 50 – 63 25	63 7	63 5	2
$ps' = 110 : 10\bar{1}$	—	—	—	71 34	—
$pq = 110 : 0\bar{1}\bar{1}$	35	116 35 – 117 11	116 53	116 55	2
$r'o' = \bar{2}01 : \bar{1}11$	11	34 42 – 35 15	34 58	35 2	4
$o'm = \bar{1}11 : 0\bar{2}1$	1	—	36 56	36 54	2
$mp = 0\bar{2}1 : 110$	1	—	55 47	55 49	2
$o'p = \bar{1}11 : 110$	12	92 32 – 92 58	92 48	92 43	5
$pr' = 110 : \bar{2}0\bar{1}$	29	52 2 – 52 28	52 12	52 15	3
$r'p = \bar{2}01 : 110$	28	127 25 – 128 2	127 48	127 45	3

Total number of measurements, 464.

Volume

Relative Density.—Four determinations with independent material afforded the following results —

Weight of salt employed	Sp gr at 20°/4°
5·2958	3·1148
5·5387	3·1175
5·1525	3·1126
5·5317	3·1164
Mean	3·1153

Molecular Volume $\frac{M}{d} = \frac{724·0}{3·1153} = 232·40$

Distance Ratios.—Combination of this molecular volume with the axial angle and axial ratios already given, affords the following distance ratios —

$$\chi : \psi : \omega = 6·3860 : 8·7311 : 4·3402$$

Optics

Orientation of Axes of Optical Ellipsoid.—The plane of the optic axes (binormals) is again the plane of symmetry The sign of the double refraction is also still positive as for the potassium and rubidium salts

Stauroscopic observations carried out with two section plates ground parallel to the symmetry plane gave the following extinction angles with reference to the normal to the basal plane —

Section 1	6° 7'
Section 2	5 26
Mean	5 46

The direction is in front of the normal nearer to the inclined morphological axis *a* This axis of the optical indicatrix is the second median line The first median line is accordingly situated in the acute angle of the morphological axes *ac* and is inclined 5 46 to the axis *a* The second median line lies in the obtuse angle *ac*, and is inclined 21° 57 to the vertical axis *c*

Refractive Indices.—The results of the refractive index determinations with six prisms, ground on crystals from different crops, are presented in the accompanying table

The intermediate refractive index of cæsium zinc selenate, corrected to a vacuum, is expressed to near F, for any wave length λ, by the following formula —

$$\beta = 1·5187 + \frac{704\ 232}{\lambda^2} - \frac{2\ 877\ 600\ 000\ 900}{\lambda^4} +$$

The α indices are reproduced with precisely equal accuracy if the constant 1·5187 is diminished by 0·0036; owing to slightly increased dispersion exhibited by the γ indices, these values are reproduced with slightly less accuracy when the constant is increased by the average amount of 0·0050.

Alteration of Refraction by Rise of Temperature.—Measurements of the prism angle and minimum deviation at 60°, indicated that the refractive indices are reduced by 0·0015 for 45° rise of temperature (15° to 60°).

Axes of the Optical Ellipsoid.—These values are as follows:—

Axes of optical indicatrix: $\alpha : \beta : \gamma$ = 0·9977 : 1 : 1·0033.
Axes of optical velocity ellipsoid: $\mathfrak{a} : \mathfrak{b} : \mathfrak{c}$ = 1·0024 : 1 : 0·9968.

Refractive Indices of Cæsium Zinc Selenate.

Index	Light	Prism 1.	Prism 2.	Prism 3	Prism 4	Prism 5.	Prism 6	Mean.
α Vibr par. 2 M.L	Li	—	1·5285	1·5287	1·5296	1·5293	—	1·5290
	C	—	1·5290	1·5292	1·5302	1·5298	—	1·5295
	Na	—	1·5321	1·5323	1·5332	1·5327	—	1·5326
	Tl	—	1·5353	1·5356	1·5362	1·5360		1·5358
	F	—	1·5395	1·5396	1·5404	1·5402	—	1·5399
	G	—	1·5454	1·5456	1·5461	1·5461	—	1·5459
β Vibr. par symm. axis	Li	1·5323	—	1·5326	—	1·5327	1·5328	1·5326
	C	1·5327	—	1·5332	—	1·5332	1·5334	1·5331
	Na	1·5359	—	1·5361	—	1·5364	1·5365	1·5362
	Tl	1·5391	—	1·5396	—	1·5395	1·5395	1·5394
	F	1·5432	—	1·5434	—	1·5437	1·5437	1·5435
	G	1·5491	—	1·5497	—	1·5496	1·5498	1·5495
γ Vibr. par. 1 M.L	Li	1·5371	1·5371	—	1·5381	—	1·5376	1·5375
	C	1·5377	1·5377	—	1·5385	—	1·5381	1·5380
	Na	1·5409	1·5408	—	1·5418	—	1·5411	1·5412
	Tl	1·5443	1·5444	—	1·5451	—	1·5445	1·5446
	F	1·5484	1·5486	—	1·5493	—	1·5488	1·5488
	G	1·5544	1·5547	—	1·5551	—	1·5549	1·5549

Molecular Optical Constants.—These are as under:—

Axis of optical indicatrix.		α	β	γ
Specific refraction, $\frac{n^2-1}{(n^2+2)d} = \mathfrak{n}$	C	0·0991	0·0996	0·1004
	G	0·1016	0·1022	0·1030
Molecular refraction, $\frac{n^2-1}{n^2+2} \cdot \frac{M}{d} = \mathfrak{m}$ ..	C	71·78	72·14	72·60
	G	73·58	73·98	74·58
Specific dispersion, $\mathfrak{n}_G - \mathfrak{n}_C$		0·0025	0·0026	0·0026
Molecular dispersion, $\mathfrak{m}_G - \mathfrak{m}$		1·85	1·84	1·89
Molecular refraction, $\frac{n-1}{d}M$	C	123·06	123·89	125·08

Optic Axial Angle.—The optic axial angle in air 2E is so large as to be only measurable with some difficulty, even in the cases of the largest and most perfectly transparent sections. Owing to the relatively small double refraction of all the salts of this series, sections require to have a thickness of at least a millimetre in order to afford small rings and sharp brushes. Hence, if the section is not of considerable, and not always attainable, relative breadth the brushes become obscured. Two of the sections prepared enabled trustworthy measurements to be obtained, but the others, although excellent for $2H_a$, did not exhibit adequately clear brushes in air.

Apparent Angle in Air of Cæsium Zinc Selenate.

Light	Section 1		Section 2		Mean 2E.	
	°	′	°	′	°	′
Li	162	50	165	20	164	5
C	163	12	165	33	164	22
Na	165	43	166	30	166	6
Tl	168	21	167	23	167	52

No measurements were obtainable beyond the green, the angle becoming too large.

From the results given in the accompanying table for the true angle, it will be observed that the dispersion of the optic axes is considerably greater than in the cases of the potassium and rubidium salts. To confirm its nature, and determine the dispersion of the median lines, a liquid was sought for whose refraction was the same as that of the crystals, and which was without action on them. Pure methyl salicylate fulfils these conditions, and the interference figures afforded by sections perpendicular to the first median line were observed, while immersed in a cell of this liquid. Measurements in C and F light afforded angles almost identical with those calculated from $2H_a$ and $2H_o$ as measured in monobromonaphthalene, and showing the same amount and order of dispersion.

Four pairs of section plates were employed in the determinations of the true angle.

Determination of True Optic Axial Angle of Cæsium Zinc Selenate

Light	No of section perpendicular 1st median line	Observed values of 2Ha	No of section perpendicular 2nd median line	Observed values of 2Ho	Calculated values of 2Va	Mean value of 2Va
		°		°		°
Li	1	76 27	1*a*	87 33	83 37	83 33
	2	76 12	2*a*	87 28	83 30	
	3	76 15	3*a*	87 30	83 31	
	4	76 20	4*a*	87 30	83 31	
C	1	76 23	1*a*	87 32	83 34	83 30
	2	76 [illegible]	2*a*	87 27	83 27	
	3	76 9	3*a*	87 29	83 28	
	4	76 14	4*a*	87 28	83 32	
Na	1	75 43	1*a*	87 26	83 12	83 [illegible]
	2	75 26	2*a*	87 23	83 4	
	3	75 25	3*a*	87 27	83 1	
	4	75 29	4*a*	87 24	83 5	
Tl	1	75 1	1*a*	87 18	82 50	82 43
	2	74 47	2*a*	87 18	82 41	
	3	74 49	3*a*	87 24	82 39	
	4	74 49	4*a*	87 20	82 41	
F	1	74 17	1*a*	87 9	82 26	82 14
	2	73 53	2*a*	87 11	82 10	
	3	73 55	3*a*	87 21	82 6	
	4	74 0	4*a*	87 14	82 12	

Dispersion of the Median Lines.—When the true angle is observed in methyl salicylate in white light, the brushes are seen to be highly coloured in accordance with the following scheme —

blue | red   1st M L   red | blue

The angle is thus indubitably the larger for red If the section is arranged so that the obtuse angle of the morphological axes *ac* is situated to the left, the optic axis to the right is found to be less dispersed between C and Tl light by about 10 than the left-hand axis. Hence the median lines are dispersed by about 5 between the same wave-lengths, and so that the first median line lies nearer to the morphological axis *a* for red than for green

Effect of Rise of Temperature on the Optic Axial Angle.—Repeated measurements of 2E with the best of the sections at 60°, indicated that the angle in air decreases about 3° for 50° rise of temperature

Comparison of the Three Salts.

A concise statement of the conclusions to be drawn from a comparison of the results for the three salts will be found in the Abstract published in the 'Proceedings of the Royal Society,' vol. 66, p. 248. The following comparative tables and diagrams will enable these conclusions to be clearly appreciated.

Comparison of the Morphological Angles of the Three Zinc Salts.

Angle	Potassium salt		Diff.	Rubidium salt		Diff.	Cæsium salt	
	°	′	′	°	′	′	°	′
ac = 100 001 = β	75	48	−64	74	44	−55	73	49
as = 100 101	46	19	−32	45	47	−44	45	3
sc = 101 001	29	29	—	28	57	—	28	46
cr′ = 001 $\bar{2}$01	63	12(15)	+30	63	42(53)	+54	64	36
cs′ = 001 $\bar{1}$01	38	22	—	38	24	—	38	52
s′r′ = $\bar{1}$01 $\bar{2}$01	24	50	+28	25	18	+26	25	44
r′a = $\bar{2}$01 $\bar{1}$00	41	0	—	41	34	—	41	35
ap = 100 · 110	35	52	−14	35	38	−34	35	4
pp′ = 110 120	19	28	—	19	28	—	19	28
p′b = 120 010	34	40	+14	34	54	+34	35	28
pb = 110 010	54	8	—	54	22	—	54	56
cq = 001 011	26	8	−18	25	50	−19	25	31
qb = 011 010	63	52	—	64	10	—	64	29
ao = 100 . 111	49	35	−36	48	59	−47	48	12
oq = 111 011	27	42	—	27	19	—	27	14
aq = 100 · 011	77	17	−59	76	18	−52	75	26
qo′ = 011 $\bar{1}$11	34	31	—	34	37	—	35	6
o′a = $\bar{1}$11 $\bar{1}$00	68	12	+53	69	5	+23	69	28
co = 001 111	35	11	−37	34	34	−21	34	13
op = 111 . 110	43	21	—	43	5	—	42	36
cp = 001 110	78	32	−53	77	39	−50	76	49
po′ = 110 11$\bar{1}$	56	50	—	57	43	—	58	14
o′c = 11$\bar{1}$ · 00$\bar{1}$	44	38	0(+7)	44	38(45)	+19(12)	44	57
bo = 010 111	69	51	+22	70	13	+24	70	37
os = 111 · 101	20	9	—	19	47	—	19	23
bo′ = 010 · $\bar{1}$11	65	10	+4	65	14	+8	65	22
o′s′ = $\bar{1}$11 · $\bar{1}$01	24	50	—	24	46	—	24	38
sq = 101 . 011	38	36	−34	38	2	−19	37	43
qp = 011 · $\bar{1}$10	85	26	+64	86	30	+67	87	37
ps = $\bar{1}$10 . $\bar{1}$0$\bar{1}$	55	58	—	55	28	—	54	40
s′q = $\bar{1}$01 : 011	45	15	—	45	8	—	45	21
qp = 011 : 110	64	7	−37	63	30	−25	63	5
ps′ = 110 : 10$\bar{1}$	70	38	+44	71	22	+12	71	34
r′o′ = $\bar{2}$01 : $\bar{1}$11	34	38	+16	34	49	+13	35	2
o′p = $\bar{1}$11 · 110	93	10	—	92	39	—	92	43
pr′ = 110 : 20$\bar{1}$	52	17	+15(4)	52	32(21)	−17(6)	52	15

The minutes figures in brackets in the table are the observed values in those cases where the differences between the observed and calculated values are appreciable.

The angular change is not generally directly proportional to the change in the atomic weight of the alkali metal, the maximum variation from direct proportion among the primary angles being exhibited by the prism zone, where the changes in *ap* or *bp* are as 1 to 2½.

Comparison of the Axial Ratios

For potassium zinc selenate, $a : b : c$ = 0·7458 : 1 : 0·5073
„ rubidium „ „ $a : b : c$ = 0·7431 : 1 : 0·5019
„ cæsium „ „ $a : b : c$ = 0·7314 : 1 : 0·4971.

Comparison of the Relative Densities.

Potassium zinc selenate ...	2·5537	
		Diff. 0·3067
Rubidium „ „ ...	2·8604	
		Diff. 0·2549
Cæsium „ „ ..	3·1153	

The difference between the densities of potassium zinc and rubidium zinc sulphates is 0·343, and between those of the latter salt and cæsium zinc sulphate 0·283.

Comparison of the Molecular Volumes.

Potassium zinc selenate	210·13	
		Diff. 9·77
Rubidium „ „	219·90	
		Diff. 12·50
Cæsium „ „	232·40	

The differences for the two replacements in the zinc double sulphates were 9·52 and 12·68.

Comparison of the Distance Ratios.

	χ.	Diff.	ψ.	Diff.	ω.	Diff.
KZn selenate ...	6·1941		8·3054		4·2133	
		1121		1809		460
RbZn „ ...	6·3062		8·4863		4·2593	
		798		2448		809
CsZn „ ...	6·3860		8·7311		4·3402	
		1919		4257		1269

These ratios may be simplified by referring them to ψ for KZn selenate as unity, when the values for that salt become identical with the axial ratios. These ratios are as under:—

	χ.	Diff.	ψ.	Diff	ω.	Diff.
KZn selenate ...	0 7458		1		0·5073	
		135		218		55
RbZn „ ...	0·7593		1·0218		0·5128	
		96		295		98
CsZn „ ...	0·7689		1·0513		0 5226	
		231		513		153

Comparison of the Orientations of the Optical Indicatrix.

Inclinations of Axis α of Indicatrix to Vertical Axis c.

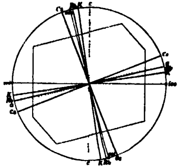
	°	′		°	′
For potassium zinc selenate	9	9			
			Diff.	4	4
„ rubidium „ „	13	13			
			„	8	44
„ cæsium „ „	21	57			

The rotation of the ellipsoid is clearly illustrated by fig. 2.

Comparative Table of Refractive Indices.

Index	Light.	KZn selenate.	RbZn selenate.	CsZn selenate
α Vibr. par. 2 M.L.	Li	1 5087	1 5129	1 5290
	C	1 5092	1·5134	1·5295
	Na	1·5121	1·5162	1·5326
	Tl	1·5151	1·5194	1·5358
	F	1·5189	1·5233	1·5399
	G	1·5244	1·5288	1·5459
β Vibr. par. symm. axis.	Li	1·5146	1·5188	1·5326
	C	1·5151	1·5193	1·5331
	Na	1 5181	1·5222	1·5362
	Tl	1·5212	1·5258	1·5394
	F	1·5252	1·5293	1·5435
	G	1·5307	1·5351	1·5495
γ Vibr. par. 1 M.L.	Li	1·5297	1·5294	1·5375
	C	1·5302	1·5299	1·5380
	Na	1·5335	1·5331	1·5412
	Tl	1·5369	1·5365	1·5446
	F	1·5410	1·5405	1·5488
	G	1·5471	1·5466	1·5549

FIG 2.



The best mode of comparing the refraction of the three salts is probably to take the mean of all three indices, for an intermediate wave-length, of each salt, and to set these mean indices side by side. Such values for sodium light are given below.

Mean refractive index of KZn selenate for Na light	1·5212		
		Diff. 26	
„ „ RbZn „ „	1 5238		
		„ 129	
„ „ CsZn „ „	1·5367		

The mean refractive indices of the K, Rb, and Cs zinc sulphates are 1·4859, 1·4897, and 1 5054. The differences, 38 and 157, are thus reduced when sulphur is replaced by selenium

Comparison of Double Refraction, Naγ – α

For KZn selenate . . .	0·0214	
		Diff 45
„ RbZn „	0 0169	
		„ 83
„ CsZn „	0 0086	

The difference for the first replacement, 45, is thus nearly double that of the mean refraction, 26. This explains why, in spite of the general progression in refraction, the γ indices of RbZn selenate are slightly less than those of the KZn salt.

The relations of the refractive powers of the three salts are concisely expressed by the axial ratios of the optical ellipsoid, which are compared beneath, and even more clearly by the continuous curves in fig. 3, which express them graphically.

Comparison of the Optical Ellipsoids

Optical Indicatrix

	α	β	γ	Double refraction
KZn selenate	0.9960	1	1.0101	141
RbZn	0.9961	1	1.0072	111
CsZn	0.9977	1	1.0033	56

Optical Velocity Ellipsoid

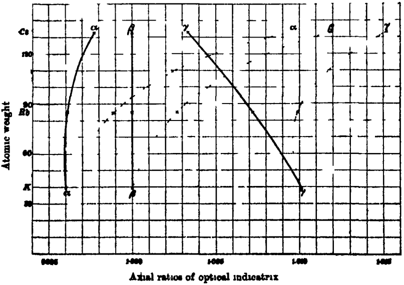
	a	b	c	
KZn selenate	1.0040	1	0.9900	140
RbZn	1.0039	1	0.9929	110
CsZn	1.0024	1	0.9968	56

Comparison of the Optical Indicatrices when $\beta_{KZn} = 1$

	α		β		γ	
KZn selenate	0.9960		1		1.0101	
		28		27		2
RbZn	0.9988		1.0027		1.0099	
		108		92		53
CsZn	1.0096		1.0119		1.0152	

The last series of ratios shows the total change of the ellipsoid on passing from one salt to another and the numbers are obtained by considering the initial length of the β axis that is, its length in the potassium salt is unity They are graphically expressed by the dotted curves in fig 3

Fig 3



Besides the main conclusion regarding these axial ratios, given in the Abstract, it will be observed that a notably less change occurs in the length of the γ axis of the indicatrix than along the other two axes, when the total change is considered; this is illustrated by the less sweep of the corresponding curve of the dotted series shown in fig. 3. When the relative change only is considered the reverse is observed, the change along the γ axis being the maximum, and its curve of the continuous series in fig. 3 taking the greater sweep. The relative relationships of the three axes to one another, in the case of each salt, govern the magnitude of the optic axial angle of each salt, and the fact that the positions for the rubidium salt are intermediate, points to an intermediate optic axial angle for that salt, an expectation which the next table shows is fulfilled.

The diminution of the double refraction as the atomic weight of the alkali metal is increased is clearly shown by the ratios, and by the convergence of the curves.

Comparison of True Optic Axial Angles, $2V_a$.

Light	KZn selenate		RbZn selenate		CsZn selenate	
		′	°	′	°	′
Li	66	12	75	16	83	33
C	66	13	75	14	83	30
Na	66	15	75	8	83	6
Tl	66	17	75	2	82	43
F	66	20	74	55	82	14

The symmetry plane is the common plane of the optic axes. The double refraction is positive for all three salts, and the disposition of the median lines is also identical, subject to the rotation of the whole optical ellipsoid.

Comparison of the Molecular Optical Constants.

Specific Refraction, $\frac{n^2-1}{(n^2+2)d} = n$.

	For ray C (Hα).			For ray Hγ near G.		
	α	β	γ	α	β	γ
KZn sel.	0·1170	0·1181	0·1210	0·1199	0·1211	0·1242
	119	119	130	121	122	134
RbZn sel.	0·1051	0·1062	0·1080	0·1078	0·1089	0·1108
	60	66	76	62	67	78
CsZn sel.	0·0991	0·0996	0·1004	0·1016	0·1022	0·1030

Molecular refraction, $\frac{n^2-1}{n^2+2} \cdot \frac{M}{d} = m$.

	For ray C (Hα)			For ray Hγ near G		
	α	β	γ	α	β	γ
KZn sel	62·76	63·37	64·93	64·33	64·98	66·63
	3·37	3·40	2·99	3·47	3·49	3·05
RbZn sel	66·13	66·77	67·92	67·80	68·47	69·70
	5·60	5·37	4·77	5·78	5·51	4·88
CsZn sel	71·73	72·14	72·69	73·58	73·98	74·58

Specific Dispersion, $n_G - n_C$.

	α	β	γ
KZn sel. .	0·0029	0·0030	0·0032
RbZn sel . .	0·0027	0·0027	0·0028
CsZn sel	0·0025	0·0026	0·0026

Molecular Dispersion, $m_G - m_C$.

	α	β	γ
KZn sel ……	1·57	1·61	1·72
RbZn sel	1·67	1·70	1·78
CsZn sel. ….	1·85	1·84	1·89

Molecular Refraction (Gladstone), $\frac{n-1}{d} M$

	α	β	γ
KZn sel	107·00	108·24	111·41
	5·90	5·96	5·12
RbZn sel.	112·90	114·20	116·53
	10·16	9·69	8·50
CsZn sel	123·06	123·89	125·03

The rules stated in the Abstract regarding specific and molecular refraction are independent of the wave-length, and whether they are calculated by the formulæ of Lorenz or Gladstone and Dale. They are also independent of temperature, for it has been shown with regard to each salt that the refraction is diminished by rise of temperature, and the density, the other factor in the calculation, is naturally affected in the same direction by increase of temperature.

The replacement of sulphur by selenium in these zinc double salts is accompanied by an increase of molecular refraction of 7·0– 7·4 Lorenz units or 13·0—13·9 Gladstone units for the ray C, according to the direction compared. The increase due to each atom is thus 3·5—3·7 or 6·5—6·9 units. The values derived from a comparison of the simple sulphates and selenates were 3·4—3·8 or 6·2—7·2. The mean values derived from the two series are thus identical.

In the next communication the magnesium group of salts will be described.

"Certain Laws of Variation. I. The Reaction of Developing Organisms to Environment." By H. M. VERNON, M.A., M.D., Fellow of Magdalen College, Oxford. Communicated by Professor E. RAY LANKESTER, F.R.S. Received March 7,—Read March 29, 1900.

In a former paper* it was shown that the ova of the Echinoid *Strongylocentrotus lividus* were extraordinarily sensitive to their environmental conditions at the time of impregnation. For instance, by keeping the mixed ova and spermatozoa in water at about 26° or 8° C. for an hour, the plutei obtained after eight days' development were some 5 per cent. smaller than those from ova kept at about 20° at the time of impregnation. It was even found that the effect produced was nearly as great if the time of subjection to the abnormal temperature were reduced to one or three minutes, though if reduced to ten seconds it was not so great. This latter result was probably due to the time being insufficient for all the ova to become impregnated at the abnormal temperature.

These observations have now been repeated and confirmed, and in addition others have been made upon the reaction of the ova to environment in the later stages of their development. It has thereby been found that the degree of this reaction diminishes in more or less regular proportion from the time of impregnation onwards.

The method of experiment is fully described in the above-mentioned paper, so it will be sufficient to state here that it consists in shaking pieces of the ovaries and of the testes of several specimens of the Echinoid in small beakers of water, and then bringing portions of the contents to the required abnormal temperature. These portions are then mixed, and after an hour the temperature is gradually brought to the normal by floating the beakers in large vessels of water. The now impregnated ova are then poured into covered jars holding 2½ to 4 litres of water, and after eight days the plutei into which they develop are killed and preserved, and measured under the microscope with a micrometer eye-piece in groups of fifty. In addition to these plutei, others are obtained in each case from ova impregnated at a normal temperature, but allowed in all other respects to develop under similar conditions. These constitute the normal or standard larvæ, from which the variations in the mean size of the other larvæ are calculated. The particular dimension measured was the length of the calcareous skeleton of the "body" of the larva, that of the arms as a rule not being determined in the present research.

The results obtained, both in the old and the present series of experiments, are given in the accompanying table those of the old series being the means of several observations —

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* 'Phil. Trans.,' B, 1895, p. 577.

Number of observations		Time of exposure to abnormal temperature	Temperature of impregnation of		Percentage diminution in size produced
			Normal larvae	Abnormal larvae	
				°	
Old series	8	1 hou	19 0	8 7	4 2
	5		19 9	25 ›	5 4
	4	1 or 3 mins	19 6	8 0	3 3
	3		19 8	25 5	5 1
	4	10 secs	19 4	7 7° or 25 7°	1 7
New series	1	1 hour	14 2	1 0	3 6
	1			8 3	4 2
	1			25 5	0 4
	1			27 7	6 0

Here it will be seen that, on an average a more unfavourable effect was produced by exposure of the ova to a temperature of about 25 5° for an hour or a minute at the time of impregnation than to one of about 8 Thus 5 7 per cent diminution was produced in the 10 observations at 25 5°, and only 3 9 per cent in the 14 at about 8

All these observations were made on the pluteus of *Strongylocentrotus lividus* A further series was also made with the pluteus of *Sphaerechinus granularis* This was found to be a somewhat less variable and less reactive organism, but even in its case a most distinct effect was produced The dimension measured in this larva was likewise the body length * The results obtained were the following —

Time of exposure to abnormal temperature	Temperature of impregnation of		Percentage diminution in size produced
	Normal larvae	Abnormal larvae	
	°	°	
1 hour	17 4	12 3	7 7
,,	19 5	10 9	1 1
	18 8	10 4	4 6
,,	22 8	12 0	2 6
	22 1	27 1	2 5
5 minutes			4 8
1 hour	20 9	26 7	0 9
5 minutes			0 9

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* For figures of these larvae, *vide* 'Phil Trans B 1898, p 468

Larvæ impregnated for an hour at about 11° were, on an average, 4·0 per cent. smaller than the normal, or practically the same as in the case of *Strongylocentrotus* larvæ. An abnormally high temperature does not seem so effective, however, judging from the few results available Thus one hour's exposure to about 27° caused only 1·7 per cent diminution, and five minutes' exposure 2·6 per cent. It should be mentioned that both in this case and that of *Strongylocentrotus*, the conditions of the short-time exposure experiments differed in one respect from the others, as the beakers of abnormally cooled or warmed ova were poured directly into jars of water at normal temperature, and were not first gradually warmed or cooled That the shock of this sudden change of temperature cannot be held accountable for much of the effect produced, is proved by the fact that in those experiments in which the time of exposure was reduced to ten seconds, only 1·7 per cent diminution in the size of the larvæ was produced altogether.

Experiments were now made to determine the effect of exposure to abnormal temperatures during later stages of development. In each case all the ova were kept for the first hour during impregnation at the same temperature, and were then divided up into two portions, which were poured into jars of water at different temperatures. In the first experiment, made in March, the temperature of the Aquarium tank water was on an average 12·9°, or distinctly low. Some of the ova, after an hour's impregnation at 13·2°, were accordingly poured into a jar of water kept at 22°. At various later periods the contents of this jar were stirred up, and portions of it poured into smaller jars, which were then transferred to the tank of running water at 12·9°. The temperature of 22°, which previous experiments had shown to be about the most favourable for the development of the larvæ, was maintained practically constant by keeping the jar in a larger vessel of water, which in its turn rested on the top of a water-bath warmed very slightly by means of a gas flame provided with a regulator. The results obtained in this experiment are given in the following table, the body length of the normal larvæ, or those kept at 12·9° during the whole of their development, being taken as 100:—

Conditions.	Size	Percentage increase per hour.	During ho
Normal larvæ (12·9°) ……	100·00		
1—11 hours at 22° ………	99·96	nil	1—11
1—28 ,, ,, ………	110·86	0·40	1—28
1—71 ,, ,, ………	116·25	0·125	28—71

For some unaccountable reason, the larvæ developing from ova kept only ten hours at 22° were apparently not affected. As subsequent results will show, this was doubtless due to some error. Those from ova kept from the end of the 1st to the end of the 28th hours were, however, increased nearly 11 per cent., or on an average 0·40 per cent. per hour for each of these hours. The larvæ from ova kept seventy hours at 22° were increased by about 16 per cent., so the additional forty-three hours produced an extra increase of 5·39 per cent. On an average, therefore, the increase during the 29th to 71st hours amounted to only 0·125 per cent. per hour, or less than a third as much as for the earlier period.

In the next experiment, made in April, the temperature of the tank water was 13·8°. In this case we see that by keeping the developing ova during the 1st to 8th hours at 23°, the larvæ were increased 7·0 per cent. in size, or 1·08 per cent. per hour. For the 1st to 19th hours the increase was 11·6 per cent., or, on an average for the 8th to 19th hours, 0·37 per cent. per hour. Again, therefore, the increase per hour for the later period is only about a third that for the earlier. After the 19th hour, apparently no further increase in size was produced. Probably this is an error, but in any case the effect must have been very slight. It should be pointed out that there is probably in almost every case a possible experimental error of some 2 per cent. in the determination of the growth of these larvæ, and occasionally, as we saw in the preceding experiment, this error may for some unknown reason be considerably greater.

Conditions.	Size.	Percentage increase per hour.	During hours
Normal larvæ (13·8°)	100·00		
1—8 hours at 23°	107·55	1·08	1—8
1—19 " "	111·60	0·37	8—19
1—43 " "	109·76	0	19—43
1—193 " "	110·88	0	19—192

In the next experiment only approximate results can be calculated. Thus larvæ were grown at respectively 13·3° and 20·3° during the whole period of development, but some of them were also preserved and measured after only 3½ days' growth. The following values were obtained:—

	3½ days.	8 days.
Kept at 13·3°	88·28	100·00
" 20·3°	109·63	111·98

Here we see that the larvæ grown only 3½ days at 20·3° are 9·63 per cent. larger than those grown 8 days at 13·3°. If they had been kept an additional 4½ days at 13·3° they would doubtless have grown somewhat more. The increase for the 1st to 84th hours is therefore somewhat more than this 9·63 per cent., though less than 11·98 per cent. Let us take it as 10·80 per cent., or 0·130 per cent. per hour. Again we see that the larvæ kept 8 days at 20·3° are only 2·35 per cent. larger than those kept 3½ days at this temperature. Hence the maximum effect capable of being produced by the more favourable temperature during this 84th to 192nd hour must be somewhat less than 0·022 per cent. per hour, or not a fifth of that produced in the earlier period.

The next experiment was made in July. After an hour's impregnation at 22·7°, some of the ova were poured into a jar of water which stood in another jar which was surrounded by water and ice. By this means the developing ova were kept at about 12°. Every few hours the water was stirred up and portions of it poured into jars, which were then transferred to the tank water. This had a mean temperature of 22·5°. The following results were obtained .—

Conditions.	Size	Percentage diminution per hour.	During hours
Normal larvæ (22·5°)…. ..	100·00		
1—6 hours at 12°……….	93·61	1·28	1—6
1—10 „ „ ……….	92·37	0·31	6—10
1—21 „ „ ……. …	90·09	0·21	10—21

Here we see that the effect produced during the 1st to 6th hours was four times as great as that during the 6th to 10th hours. In another experiment some ova, which had been impregnated at 1° C., and had thereby given rise to larvæ 3·6 per cent. smaller than the normal, were kept for the next eight hours at 6° The larvæ resulting therefrom were still 9·34 per cent. smaller, or, on an average, were diminished 1·17 per cent. for each hour of exposure to the abnormal temperature.

As it was found somewhat troublesome to keep a considerable volume of water some twelve or fifteen degrees below that of the atmosphere for many hours, the rest of the observations were made on the effects of keeping the developing ova at a higher temperature than the normal. As the temperature of the air in the summer months at Naples, where these experiments were made, varies but little from day to day, it was easy to keep the water in a tank holding about 30 litres at a practically constant temperature throughout the experiment. In fact, it did not vary more than 0·3° or 0·5° at the most. In such a tank, if left uncovered, the temperature of the water was found to fall by evaporation to about 25°, or about 2° lower than that of the atmospheric temperature. By covering it up, this could be diminished if wished, and small quantities of hot or cold water could be added to bring the temperature to exactly what was required. The room in which this tank was kept was shut up closely at night so as to prevent cooling.

In the first experiment some of the ova, after an hour's impregnation at 22·2°, were kept for varying periods in this tank of water at 26·0°, and portions of them transferred in smaller jars to Aquarium tank water at a mean temperature of 23·5°. The following results were obtained —

Conditions.	Size.	Percentage variation per hour.	During hours
Normal larvæ (23·5°) ……	100·00		
1—4 hours at 26° ……….	88·33	−3·89	1—4
1—8 " " ………	88·90	+0·14	4—8
1—12 " " ……….	94·56	+1·42	8—12
1—22 " " ……….	99·31	+0·47	12—22
1—144 " " ……	98·43	−0·007	22—144

Here we see that three hours' exposure of the developing ova to a temperature of 26° produced a diminution of 11·7 per cent. in the size of the larvæ. Further exposure, on the other hand, not only failed to produce a further diminution of size, but gave an actual increase, which gradually became more and more marked. It is obvious, therefore, that a temperature of 26°, though harmful to the ova in their earlier stages of development, becomes advantageous in the later stages. The reason of this will be made evident further on. It might be thought at first sight that this and other similar experiments in which the environment produces a varying effect, could be of no use in deciding the question under discussion. By judicious selection of certain of the values, however, useful results are obtainable. Thus, in the present instance, we see that by the end of the 8th hour the favourable action of the high temperature has already established itself, and it remains established from that time onwards. All results obtained after this period are, therefore, of value, and the figures which show that the effect produced between the 8th and 12th hours is three times as great as that between the 11th and 22nd hours, are genuine ones. The apparent slight diminution of size occurring between the 22nd and 144th hours is doubtless due to experimental error. In all the observations made during the summer months the larvæ were killed and preserved after only six days' growth, instead of eight. This was because they practically reach their maximum size in this period, the rate of growth being so much greater than at the lower temperatures experienced in the spring.

In the next experiment, the adverse effect produced during the first few hours' exposure was extraordinarily great, so that the favourable influence of the later hours was only very partially able to counteract it. Thus, during the 1st to 4th hours, the diminution effected was no less than 6·45 per cent. per hour. There was even a slight additional diminution during the next 3½ hours, but after that the increase in size noticed in the above experiment set in. It is probable that an unfavourable effect persisted even to the first portion of the 7½ to 11 hour period, as the percentage increase per hour is considerably less than one would expect. The only values which are unequivocally genuine are, therefore, the last two. From these we see that the effect produced between the 22nd and 44th hours is only an eighth of that between the 11th and 22nd hours.

Conditions.	Size.	Percentage variation per hour.	During hours
Normal larvæ (24·2°)	100·00		
1—4 hours at 26·0°	80·64	−6·45	1—4
1—7½ " "	79·24	−0·40	4—7½
1—11 " "	80·21	+0·28	7½—11
1—22 " "	84·25	+0·20	11—22
1—144 " "	87·27	+0·025	22—144

It was thought that perhaps the very marked diminution produced in the size of the larvæ might be in part due to the rather sudden changes of temperature to which the developing ova were subjected. These changes were not, as a matter of fact, by any means remarkably sudden, as the water in which the ova were placed after impregnation took about fifteen minutes to attain its temperature of 26°, whilst the reverse change from 26° to 24·2°, the temperature of the tank water in this experiment, took about ten minutes. Still, to test this supposition, some of the ova used in this experiment were subjected to several changes of temperature. Thus, one portion, directly after the first hour's impregnation, was kept three hours at 26°, then seven hours at 24·2°, then fourteen hours at 26°, and the remainder of the time at 24·2°. The size of the larvæ obtained therefrom was 85·89, or, if anything, somewhat larger than one would have expected. Another portion of the ova was kept for the 1st to 7½th hours at 24·2°, the 7½th to 22nd hours at 26·0°, and the remainder of the time at 24·2°. The size of these larvæ was 94·93, or 5 per cent. less than the normal. One would have expected them to be if anything slightly larger than the normal, as they were kept at the lower and favourable temperature during the first 6½ hours. Still, these two experiments, taken together, show that the effect of even several changes of temperature can only be slight.

Still, again, it was thought that the vigorous stirring of the water which was necessary in order to distribute the organisms evenly through it previous to withdrawal of a portion, might perhaps exert a retarding influence on development However, this was evidently not so, as, in two experiments, in which the water was absolutely unstirred throughout, the resulting larvæ varied by respectively + 2·0 and − 2·6 per cent from those derived from frequently-stirred water

In the last experiment to be described the developing ova were kept at 25° and not at 26°. Consequently, the diminution produced in the size is not so great. In this case, also, two parallel series of observations were made, one with *Strongylocentrotus* ova as usual, and another with the ova of *Sphærechinus granularis*. In the former case, the unfavourable effect of the high temperature persisted till the end of the 9th hour, and, in the latter, until at least the 21st hour, and possibly even later. The *Sphærechinus* ova did not react so much to the environment as the *Strongylocentrotus*, just as was found to be the case in the experiments on the effect of temperature at the time of impregnation. Still these observations on *Sphærechinus*, as far as they go, more or less support the conclusion drawn from the *Strongylocentrotus* experiments, viz., that there is a diminishing reaction to environment as the stages of development progress.

Conditions.	*Strongylocentrotus* larvæ		During hours	*Sphærechinus* larvæ	
	Size	Percentage variation per hour		Size	Percentage variation per hour
Normal larvæ (23·3°)	100·00	..	..	100·00	
1—4 hours at 25° ..	93·95	−2·02	1—4	97·56	−0·81
1—9 " "	92·64	−0·26	4—9	94·20	−0·67
1—21 " "	97·68	+0·42	9—21	93·96	−0·02
1—144 " "	96·88	−0·007	21—144	95·17	+0·009

We see, then, that in all of these four sets of observations the originally unfavourable influence of the high temperature is later on converted into a favourable one. What is the cause of this? No absolute explanation was arrived at, but some observations made on the maximum or death temperatures of the developing ova gave a very satisfactory partial explanation. In these observations portions of the water containing the ova in various stages of development were placed in a beaker, and this was placed in a larger beaker of water which was gradually warmed. The beaker containing the ova was continuously stirred with a thermometer, and when the required temperature had been reached it was removed from the warm water and quickly cooled down by a stream of cold water. After keeping for twenty-four hours, corrosive sublimate was added to kill off any of the embryos still surviving, and they were all collected in a small glass cell and examined under the microscope. From the different stages of development attained by the developing ova killed at the time of heating, and those only killed twenty-four hours later by the sublimate, one could easily determine the effect of the various degrees of high temperature.

For ova at the time of impregnation the fatal heat temperature is probably about 28·5°. Thus only 31 per cent. of some ova heated to 27·7° at the time of impregnation were found to have developed to normal blastulæ twenty-four hours later, whereas some of the same ova impregnated at a normal temperature (14·2°) were all found without exception to have reached the blastula stage On the other hand, in another case not a single ovum out of a number heated to 30° at the time of impregnation showed any sign of normal development twenty-four hours later.

As regards subsequent stages, portions of some developing ova, four hours after impregnation, were heated to respectively 29°, 32°, 35, and 38°. Next day all the embryos heated to 29° and 32° had nearly or quite arrived at the pluteus stage, whilst none of those heated to 35° or 38° had got further than the half-formed blastula stage. The fatal temperature must therefore have been between 32° and 35°, or say 32·5°. Other portions of the same stock of developing ova were heated in a similar manner twelve hours after impregnation. Next day all those heated to 29°, 32°, and 35° had arrived at the full or semi-pluteus stage, whilst all of those heated to 38° were either normal blastulæ or blastulæ just beginning to invaginate. The death temperature in this case must therefore have been about 36·5°. Still other portions of the same stock of embryos were heated to various temperatures twenty-eight hours after impregnation. They had now arrived at the free-swimming pluteus stage, and hence it was quite easy to determine by naked-eye observation what effect had been produced. Of the plutei heated to 37°, none were affected, but all of those heated to 39° sank to the bottom of the beaker in a few minutes. However, about a third of them had recovered an hour after, and all of them had recovered several hours after. None of these plutei were heated above 39°, so the actual death temperature was not determined; but other results showed that the death temperature is only slightly above the heat paralysis temperature, so one may conclude that it was in this case about 39·5°.

On heating some of the six days plutei obtained from the same stock of ova, it was found that a quarter of an hour after heating, three-fourths of those heated to 39° had sunk to the bottom of the beaker, and all of those heated to 40° and 41°. After an hour, all of those heated to 39°, and half of those heated to 40°, were free-swimming. After four hours four-fifths of those heated to 40° were free-swimming, but none of those heated to 41° had recovered. The death temperature was therefore about 40·3°.

These death temperature observations perhaps become more striking if put in tabular form. Thus:—

Stage of development	Time after impregnation.	Death temperature.
Strongylocentrotus ova	..	28·5°
Semi-blastulæ	4 hours	33·5
Blastulæ and semi-gastrulæ	12 ,,	36·5
Plutei and semi-plutei	28 ,,	39·5
Plutei	6 days	40·3

It should be remarked that the embryos used in these observations had been kept at 26° during development. Six days' plutei obtained from the same stock of ova, but allowed to develop at 23·5° instead of 26°, were found to have a death temperature of 39·3°. Thus the higher temperature of development had produced a certain amount of acclimatisation.

The bearing of these results on the curious double effect of exposure of the developing ova to high temperature is obvious. Thus, if a temperature of 29° is fatal to the vitality of ova at the time of impregnation, the temperatures a few degrees below this are doubtless unfavourable to development. Still lower temperatures, on the other hand, are known to exert a favourable influence. Now as in the course of development the death temperature gradually rises, one is quite justified in concluding that the lower limit of the unfavourable temperature rises too, and very probably to a more or less similar extent. In the above experiments it was found that up to the end of four hours a temperature of 26° was distinctly unfavourable to growth. During the next four hours it was more or less neutral, but after this time it was most distinctly favourable. Now the present observations show that between the 4th and 12th hours the death temperature rises about 3°, so what was an unfavourable temperature to the earlier stage of development may have become converted into a favourable one to the later stage.

Let us now return to the results on the effects of temporary subjection to abnormal temperatures. These were obtained under such a variety of conditions that one is scarcely warranted in grouping them all together, but the majority of them can be split up into three more or less homogeneous groups. In one the so-called normal larvæ were kept at about 20°, and the abnormal ones were kept for varying numbers of hours at about 8°, whereby a negative effect on growth was produced. In another, the normal larvæ were kept at 13°, the abnormal at 22°, a positive effect being produced, and in the third the normal larvæ were kept at about 24° and the abnormal ones for varying periods at about 26°, whereby a negative effect, followed by a positive one, was produced.

Normal larvæ at 20°, abnormal at 8°.			Normal larvæ at 13°, abnormal at 22°.			Normal larvæ at 24°, abnormal at 26°.		
Time of exposure in hours.	Mean time in hours	Per cent. variation in size per hour	Time of exposure in hours.	Mean time in hours.	Per cent. variation in size per hour.	Time of exposure in hours	Mean time in hours.	Per cent. variation in size per hour.
0—1	0·5	−4·14	—	—	—	0—1	0·5	−5·92
1—6	3·5	−1·29	—	—	—	1—4	2·5	−6·45
1—9	5·0	−1·17	1—8	4·5	+1·08	1—4	2·5	−3·89
6—10	8·0	−0·31	1—11	6·0	0·0	1—4	2·5	−2·09
10—21	15·5	−0·21	8—19	13·5	+0·37	8—12	10·0	+1·42
—	—	—	1—28	14·5	+0·40	9—21	15·0	+0·42
—	—	—	19—43	31·0	0·0	11—22	16·5	+0·20
—	—	—	1—81	42·5	+0·130	12—22	17·0	+0·47
—	—	—	28—71	49·5	+0·126	21—144	82·5	0·0
—	—	—	19—192	105·5	0·0	22—144	83·0	+0·025
—	—	—	84—192	138·0	+0·022	22—144	83·0	0·0

The results are arranged in this table according to the times during which the larvæ were exposed to abnormal conditions. The means of these times are also given, as comparisons are thereby rendered easier. In the first line of the left portion of the table is given the average effect produced in the ten experiments already quoted, in which the ova were kept at about 8° at the time of impregnation. (The experiments in which the time of exposure was one to three minutes have been omitted, as the effect produced in this case was probably something special, directly connected with the act of impregnation.) The results in this group of observations show a fairly regular and very rapid diminution in the effect produced on the size of the larvæ with progress in development, but unfortunately they extend only to the 21st hour. The results in the middle portion of the table extend to the 192nd hour, but they are very irregular. Nevertheless they also, on the whole, show a rapidly diminishing effect. The results in the right portion of the table bear out this result more fully. In the first line is given the mean of the seven observations in which the ova were kept at 26° at the time of impregnation. In the next three observations in the table, in which the mean time of exposure was 2·5 hours, the mean effect produced was 4·1 per cent. In the 10th hour it was 1·4 per cent., in the 15th to 17th hours on an average 0·36 per cent., and in the 83rd hour on an average only 0·008 per cent. All the observations made, therefore, whether taken in the small groups in which they were originally obtained, or taken collectively, agree in showing that the effect of temperature on the growth of an organism diminishes very rapidly from the time of impregnation onwards It is to be noticed that the effect produced was as far as could be ascertained, a permanent one At least it persisted to the full larval growth of the organisms for the larvæ were found to practically cease growing after six to eight days development How much would have persisted through the metamorphosis to the adult Echinoid stage is, of course, another matter

It seems highly probable that what is true for temperature is true for other environmental conditions, and that future research will justify one in assuming the existence of a definite Law of Variation This might be worded as follows ' *The permanent effect of environment on the growth of a developing organism diminishes regularly and rapidly from the time of impregnation onward*

It is necessary for me now to make one serious criticism of all these observations one which I regret to say did not occur to me till after they had been completed and when it was too late for me to put it to a proper experimental test This criticism depends on the obvious fact that all organisms must be confined within comparatively narrow limits in their powers of growth so that for instance supposing a *Strongylocentrotus* plutous under average conditions attains a size of 100, then probably under no conditions whatsoever could it be made to attain a larger size than 120 to 125 or a smaller one than 80 to 75 Thus in the most extreme variation noticed in any of the numerous observations made on these larvæ the range ran from 19 2 per cent above the normal to 18 0 per cent below it Now supposing that during the first hours of development an embryo is placed under especially favourable conditions then it may happen that thereby it is stimulated to undergo all or nearly all, the increased growth of which it is capable In subsequent hours, therefore, little if any more favourable effect may be produced, simply because the organisms from their very nature are unable to show it

If this principle be examined in relation to the present experiments, I think it can be shown, however, that though no doubt the relation between the reaction of the organism during the earlier hours to that in the later hours has thereby been exaggerated yet that there still remains plenty of evidence behind to prove that the diminishing reaction to environment exists in addition

Let us first consider the three series of experiments in which the developing ova were kept at 26° or 25° Here a diminution of size amounting to from 7 36 to 20 76 per cent is produced by the first few hours' exposure to high temperature, so that after this, when the environment begins to exert a favourable influence, we know that it has at least this range of growth capacity at its disposal, plus whatever amount of increased growth one might have been able to effect in the "normal" larvæ, by exposing them to the most favourable conditions of growth possible. Now we see that in no case did the favourable environment succeed in forcing on the growth of the larvæ to that of the original normal larvæ, so there was always plenty of growth capacity at its disposal.

In the experiment in which the ova were kept at 12° instead of 22·5°, there is no doubt that the larvæ could have been diminished at least 10 to 15 per cent. more if the conditions had only been sufficiently unfavourable and sufficiently long continued. Thus in the above-mentioned paper it is shown* that larvæ kept during the whole period of development at 10° instead of about 20° are diminished in size by no less than 24 per cent.

We see, therefore, that in two of the different methods adopted for acting on the larvæ there was always a considerable amount of growth capacity still present. This may have been true also for the third method, though in this case one cannot prove it. Now we have seen in the above tables that the reaction in the latest periods of development was not a hundredth or even a five hundredth part of that in the first hour, and hence, even admitting the growth capacity was diminished, there can be no doubt whatever that the sensitiveness of the organism to the environment undergoes an enormous gradual diminution.

In order to determine exactly the sensitiveness of the developing ova to environment during the various stages of growth one should keep various portions of them at the normal temperature for the first three, six, &c., hours, and then expose them to the abnormal temperature for a few hours. Then they should be transferred, for the remainder of their developmental period, to the normal temperature. In this way there would always be the same amount of growth capacity for the environment to work upon, and so the effects obtained for the various periods would accurately express the true capacity for reaction.

The Effect of other Environmental Conditions

It is obvious that in order to demonstrate the principle under discussion, almost any sufficiently powerful condition of environment might have been chosen. Temperature was hit upon first as being the most convenient one, but further series of experiments were made with another condition also, that of salinity of the water. It has been shown† that growth of the larvæ in water of a certain dilution may increase the size by as much as 15·6 per cent., whilst growth in pure sea water, instead of Aquarium tank water may increase it by as much as 19 2 per cent * As a rule, however the effect produced is not so great as this

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* Phil. Trans. B. 1898, p. 481.

† Phil. Trans. B. 1895, p. 587.

The developing ova after impregnation for one hour under normal conditions in ordinary Aquarium tank water were kept for various periods in diluted sea water or pure sea water and were then transferred to ordinary tank water again Once the ova have reached the free swimming blastula stage or within about five hours in the middle of the summer it is practically impossible to separate them from the water in which they are swimming In all the experiments therefore, one part of the diluted or pure sea water after vigorous stirring to distribute the embryos evenly through it was poured into ten parts of the normal water The subsequent growth of the embryos was therefore continued in tank water containing an eleventh part of the foreign water but as will soon be seen this could have made very little difference to their size

In the first experiment made in the beginning of April the developing ova were placed in diluted water made by adding 100 c c of fresh water to 1900 c c of Aquarium tank water The specific gravity of this water was found to be 1 02736 at 15 56° C, whilst that of the unadulterated tank water was 1 02869 The following were the results obtained

Normal larva (13 8)	100 00
1 6 hours in diluted water	95 44
1 12 ,,	93 55
1 24 ,,	89 02
1 192	102 08

Here we see that larvæ kept first in diluted water, and then transferred to normal water, are considerably diminished in size, those transferred after twenty four hours development being diminished by no less than 11 per cent

The next experiment was made in July when the temperature of the water during development was 21 5 , or nearly 8° higher than in the April experiment The following values were obtained —

Normal larva (21 5)	100 00
1 6 hours in diluted water	96 90
1—11 ,, ,,	96 63
1 24 ,,	103 28
1—144 ,, ,,	103 66

In this case the larvæ reached their minimum size after ten hours' growth, and then so rapidly increased that fourteen hours later they were 3 per cent larger than the normal

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* 'Mittheilungen a d Zool Stat zu Neapel,' vol 13, p 376

The next and last experiment was made in August, when the temperature of the water was on an average 24·5°. In this case the larvæ were first kept in pure sea water collected several kilometres from the shore. The specific gravity of this water was 1·02868 at 15·56°, that of the normal tank water being 1·02901.

Normal larvæ (24·5°)	100·00
1—4 hours in pure sea water	90·38
1—8 ,, ,, ,,	93·61
1—12 ,, ,, ,,	96·98
1—22 ,, ,, ,,	97·04
1—144 ,, ,, ,,	103·61

Here we see that the larvæ were reduced to their minimum size by only three hours' development in pure sea water, and that longer treatment produced a more and more favourable effect, though only those larvæ kept for the whole period of development in the pure water were larger than the normal.

We see, therefore, that in each of these series of experiments, though the ultimate effect of the diluted or pure sea water was a favourable one, yet a temporary immersion in it was always unfavourable. The times of production of the maximum diminution of size were respectively twenty-four, ten, and three hours, or apparently very variable. It is to be noticed, however, that these times more or less correspond with the period at which the developing ova reach the free-swimming blastula stage. Thus at a temperature of 13·8° this was found to be some twenty to twenty-four hours, whilst at a temperature of 24° it was about five hours. At 21·5° it is probably about eight hours, though no exact observations were made to determine it.

To what is this unfavourable effect upon the larval growth due? It is impossible that the pure sea water can of itself be a less favourable medium for the early stages of development than the impure tank water, and probably the same is true as regards the diluted tank water. In all probability the harmful effect is to be attributed to the shock attendant on the transference of the embryos from water of a lesser degree of salinity to that of a greater. Owing to the differences of osmotic pressure thereby set up, the tissues would immediately undergo a certain amount of shrinkage, and it is a ready assumption that their growth is thereby for a time delayed. The sensitiveness of the embryos to a change of salinity would seem to be less and less the more advanced the state of development, so that after a day or two's growth the harmful influence becomes entirely in abeyance.

The reverse process of transference of the developing embryos from more saline to less saline water does not, on the contrary, appear to be attended with any unfavourable result. Thus some of the same stock of impregnated ova used in the second of the above experiments were kept for respectively five and twenty-four hours in ordinary tank water, and were then transferred to diluted water, and kept there for the remainder of their development. The larvæ so obtained were respectively 2.6 and 2.8 per cent. larger than the normal, or but slightly smaller than the larvæ kept for the whole period of development in diluted water. In another instance, also, embryos kept for respectively twenty-three hours and two days in normal water, and for the rest of development in diluted water were 3.2 per cent. and 1.1 per cent. larger than the normal.

These experiments therefore prove that the condition of salinity is not a favourable one for the determination of the reaction to environment. Still they serve to emphasise the extraordinary sensitiveness of the embryos to their environmental conditions, and also show that this sensitiveness is much greater in the earlier stages of development than in the later ones. Only one observation was made on the effect of keeping the ova in diluted water for an hour at the time of impregnation. In this case a diminution of 2.2 per cent. was produced in the size of the larva. In the paper already mentioned, however, five experiments of this nature are recorded,* the water being of various degrees of salinity. The effects produced were respectively −4.3, +4.1, 1.8, 2.9 and −2.4 per cent., or on an average 1.5 per cent.

Summary.

By keeping the impregnated ova of the Echinoid *Strongylocentrotus lividus* for various periods during development at an abnormal temperature, and comparing the size of the larvæ into which they developed with that of larvæ allowed to grow throughout under normal conditions, it was proved that *the permanent effect of temperature on the growth diminished rapidly and regularly from the time of impregnation onwards.* For instance, it was found that exposure of the ova to a temperature of about 8° for an hour at the time of impregnation produced an average diminution of 4.1 per cent. in the size of the larvæ measured after eight days' growth; during the 4th hour after impregnation the diminution produced for each hour's exposure was about 1.2 per cent., and during the 15th hour about 0.2 per cent. In another series, exposure to a temperature of 22° produced an increase in size, this amounting to about 1.1 per cent. for each hour's exposure in the 4th hour, to 0.4 per cent. in the 14th hour, 0.13 per cent. in the 46th hour, and 0.01 per cent. in the 120th hour.

Exposure to a temperature of 26° during the first few hours of development produced a diminution of from 20.8 to 7.4 per cent.,

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* 'Phil. Trans.,' B, 1895, p. 588.

but in the later hours it produced an increase of from 4·3 to 11·0 per cent. The reaction of the organism to a constant environmental condition was thus a variable one. This is probably explicable by the fact that the temperatures necessary to kill the organisms, and presumably also those which cause an unfavourable effect on growth, rise steadily during development. Thus the death temperature is about 28·5° for unsegmented ova, 34 for blastulæ, and 40 for plutei.

The impregnated ova were also found to be much more sensitive to changes in the salinity of the water during the early stages of development than during the later ones.

"On the Diffusion of Gold in Solid Lead at the Ordinary Temperature."

By Sir W. Roberts-Austen, K.C.B., F.R.S., Professor of Metallurgy, Royal College of Science. Received April 5,—Read May 10, 1900.

In the Bakerian Lecture, "On the Diffusion of Metals,"* delivered in 1896, evidence was given to show that gold placed at the base of a column of fluid lead 16 cm. high, maintained at a mean temperature of 492°, or 166 above the melting point of lead, diffuses to the top of the column in an appreciable amount in a single day, the diffusivity expressed in centimetre-day units being 3·0. If the lead be heated, say to 251°, or 75° below the melting point of the metal, diffusion takes place at a much slower rate; it may still be readily measured, though the diffusivity is only 0·023 in centimetre-day units. In the experiments on diffusion in solid lead, the latter metal was prepared with great care, and possessed a high degree of purity. The method of preparation consisted in the reduction of carefully purified carbonate of lead by cyanide of potassium, the reduced metal being cast in carbon moulds.

It became evident that at the ordinary temperature the rate of diffusion of solid gold in solid lead must be very slow, and I stated in the Bakerian Lecture that cylinders of lead had been set aside with discs of gold affixed to their bases, in order that, after a sufficient lapse of time, the diffusion occurring at the ordinary temperature might be measured. By the month of March in the present year, four years had elapsed since the experiment began, and the time appeared to be sufficiently long to justify the attempt to ascertain how far the gold had diffused. In starting the experiments the bases of the lead cylinders were carefully brought to a smooth surface, and the discs of pure gold were specially cleaned, the discs of gold being held against the bases of the cylinders by means of clamps The laboratory in which the cylinders were placed consists of a vaulted chamber situated in the basement of the Mint and its temperature varied but little from a mean of 18 C The diameters of the cylinders were in all cases 0 88 cm their lengths varied somewhat, the longest being 25 cm At the end of the four years the discs of gold were found to be adherent to the lead The cylinders were divided into thin slices at right angles to the axes of the cylinders the first slice was approximately 0 75 mm thick but the succeeding layers were about 2 3 mm thick By the ordinary methods adopted by assayers, which were conducted with extraordinary precautions, gold was found in each of the four lower slices while only the minutest traces of gold could be found in any slice beyond the fourth from the base The amount of gold that had diffused in the different cylinders of lead was, however, not uniform The variation is probably due to difference in contact

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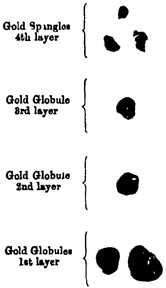
* Delivered February 20, 1896. 'Phil. Trans.,' A, vol. 187 (1896) pp. 383—415.



between the cylinders of lead and the discs of gold The results in all four experiments were, however, of the same order, and it will be sufficient to give the actual amounts of gold found in a single cylinder The richest layer was, of course, the one in direct contact with the gold, and from it a globule of gold was extracted which weighed 0 00005 gramme There is in the Mint a balance that will readily weigh such globules The gold extracted from the 2nd and 3rd layers was too small to be weighed, but the amounts could be approximately determined by measurement under a microscope Actual photographs of the gold extracted from the successive layers of a cylinder are, moreover, appended; the magnification being in all cases the same (56 diameters).

It may be thought that the amounts are but small, but from the point of view of the assayer, who is accustomed to determine minute quantities of precious metal in large masses of material, the results assume very substantial proportions. Thus the amount of gold found in the richest layer of lead represents no less than 1 oz. 6 dwts. of gold per ton, which could be profitably extracted, while the amount in even the poorest layer is 1½ dwt per ton.

The significance of these results may perhaps be made clearer if it is stated that the amount of gold which would diffuse in solid lead at the ordinary temperature in 1000 years is almost the same as that which would diffuse in molten lead in a single day, provided no more gold is supplied in either case than can be held in solution. This will serve to show how important temperature is in relation to diffusion. As an example of the relative effects of temperature on this purely physical change and on a chemical change, it may be interesting to refer to the case of the dissociation of auric chloride. At the ordinary temperature, the tri-chloride of gold is very stable though it decomposed rapidly at 180°, and my colleague, Dr. Rose,* has shown that though the decomposition of auric chloride may be perceptible at a temperature of 70°, it would nevertheless require, at that temperature, about twenty-five years for its nearly complete change into monochloride.

I believe, with Robert Boyle, that though solid gold may have its "little atmosphere," "no man has yet tried whether gold may not in time lose its weight," but the rate at which gold can possibly evaporate into the air at the ordinary temperature must be far less than that at which it diffuses into lead. This shows that the action of a solvent for the gold is necessary, and this solvent is provided by bringing gold into contact with *solid* metallic lead.

I would express my warm acknowledgment to Dr. A. Stansfield, who aids me in conducting the Metallurgical Laboratory at the Royal College of Science, for the care he has devoted to the tedious manipulation involved in these experiments. His help has given me great confidence in the accuracy of the results It may be well to add that I propose to prepare suitable cylinders of lead and gold on the lines indicated in this paper, and to offer them to the National Physical Laboratory with a view to their being examined after such a lapse of time as may be deemed fully adequate.

[*Note, May* 28.—In the Graham Lecture, delivered at Glasgow on the 18th of April last, after speaking of the diffusion of gold in solid lead,

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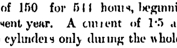
* 'Journ. Chem. Soc.,' vol. 67 (1895), p. 904.

I stated that I was "trying to ascertain whether diffusion in the solid metal is, or is not, accelerated by the simultaneous passage of a strong electric current." I again referred to the subject in answer to Lord Kelvin during the discussion which followed the reading of the present paper, and stated that the experiments were incomplete. Such experiments take a long time, and it may be well to add that the arrangement was just as is described above, except that the lead ordinarily used for assaying was employed. Two cylinders, each 0·88 cm in diameter, with gold clamped to their respective bases, were maintained at a temperature of 150 for 511 hours, beginning on the 31st of January of the present year. A current of 1·5 amperes was passed through one of the cylinders only during the whole time, the current passing from the gold to the lead. The amount of gold which had diffused into each of the lead cylinders was then ascertained by the method which has already been described. Gold was detected at a height of 7·5 mm. in the case of the cylinder through which the current had passed, while in the other case with no current it had reached a height of 10 mm., the amount of gold in each section being also greater. Subsequent experiments showed that a part at least of this difference was due to imperfection in the contact between the lead and the gold. Other experiments are now in progress in which far greater current densities are employed.

If these experiments confirm the previous one, they will show that a solution of gold in lead does act, to a small extent, as an electrolyte. The following method was adopted for ensuring contact between the gold and the lead —

My assistant, Mr. W. H. Merrett, succeeded in joining by fusion discs of gold between two cylinders of lead, as is shown in the accompanying figure. Contact between the metals is, therefore, above reproach, but it will be many weeks before the results can be recorded.



Thirteen years ago I was unsuccessful in the attempt to electrolyse a solution of gold in metallic lead by the passage of a current of 300 ampères through the molten mass.* The failure may have been due to the fact that at the high temperature produced diffusion must have been very rapid. If, therefore, separation of gold from the lead did take place, uniformity of the solution may have been restored by diffusion. I succeeded in 1895 in obtaining some evidence as to the separation of gold from its solution in metallic lead by electrolysis through a glass septum.* This is, however, only indirectly connected with the electrolysis of alloys]

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* British Association Report, 1887, p. 341.

"On Certain Properties of the Alloys of the Gold-Copper Series."

By Professor Sir W. ROBERTS-AUSTEN, K.C.B., F.R.S., and T. KIRKE ROSE, D.Sc. Received and read May 10, 1900.

[PLATE 1.]

Notwithstanding the extraordinary importance from a technical point of view of the members of this series, which constitute the gold coinages of the world, singularly little is known respecting either their molecular constitution or even their physical constants. Both the authors of this paper possess unusual facilities for studying them, and they felt that time should not be lost in beginning a systematic examination of the series. The other alloys used for coinage have, on the other hand, not been so neglected. Many years ago one of us,† in submitting his first paper to this Society, gave a curve representing the freezing points of the members of the silver-copper series. This curve, corrected in accordance with more recent work and interpreted in a modern way, proved to be one with two branches meeting at a point where the eutectic alloy of the two metals occurs. The presence of the eutectic has also been since readily detected in standard silver and in several other members of the series, and possesses a melting point of 778°. As is well known, different portions of a mass of any of the solidified alloys of the silver-copper series, except the eutectic alloy, exhibit divergences in composition which usually amount to about two or three parts in a thousand.

The gold-copper series, on the other hand, has long enjoyed a reputation for homogeneity, and it was supposed that the variations in the composition either of the alloy which contains 916 66 parts of gold in 1000, and is used for the coinage of the Empire, or of the alloy which contains 900 parts of gold in 1000, and is one adopted by the Latin Union and in the United States of America, need not exhibit greater divergences than 0·1 part in 1000. It was, moreover, believed that such a divergence was not the result of any systematic molecular grouping. This view was shaken by one of us‡ in 1895, when evidence was obtained by chemical analysis that in the case of a gold-copper alloy containing 0·2 per cent. of impurity a certain amount of the gold was driven to the inside of the mass by solidification. Corroborative evidence was subsequently obtained by the aid of the cooling curves afforded by the recording pyrometer, a description of which has already been submitted to this Society.

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* Third Report to the Alloys Research Committee, 'Proc. Inst. Mech. Engineers, 1895, p. 240.

† Roberts-Austen, 'Roy. Soc. Proc.,' vol. 23 (1874), p. 481.

‡ Rose, 'Chem. Soc. Journ.,' vol. 67, 1895, p. 552.

To decide the point finally it was desirable to show to what group of alloys the gold-copper series belongs, and in particular to determine whether the freezing points of the various alloys would lie on a single continuous curve connecting the freezing point of gold with that of copper.

Freezing point curves were accordingly taken by the recording pyrometer of a comprehensive series of alloys. In each case 100 grammes of the alloy were employed, and the thermo-couple, protected by a very thin clay tube, was inserted in the molten mass, which had been previously thoroughly stirred. The rate of cooling was prolonged as much as possible by allowing the crucible and its contents to remain in position in the gas furnace in which the melting had been effected. The freezing points of this series have, so far as we are aware, never been published, except a few at the copper end by Heycock and Neville.[*] MM. Charpy and Riche have, however, recently stated that the curve of fusibility of the alloys of gold and copper consists of two branches meeting at a point corresponding to the eutectic alloy, which, according to these experimenters, contains 55 per cent. of gold alloyed with 45 per cent. of copper, and fuses at 940.[†] This conclusion is not confirmed by the results of our experiments, which are given in the accompanying table and are plotted in the curve, fig. 1.

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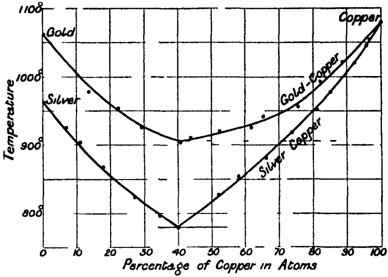
[*] 'Phil. Trans.,' A, vol. 189 (1897), p. 25.

[†] 'Administration des Monnaies et Médailles.—Rapport au Ministre des Finances,' 1899, p. xxxviii.

Fig 1

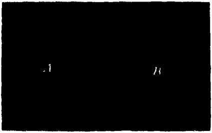


Freezing points of Alloys of Gold and Copper

Percentage weight of gold	Number of atoms of gold per 100 of alloy	Freezing point
100 00	100 00	1063° C
95 00	86 00	979
91 60	77 80	951
90 07	74 45	946
86 06	0 32	926
82 05	59 49	905
79 07	56 1	907
73 83	47 54	916
66 26	38 69	928
62 20	34 58	941
52 03	25 84	957
51 87	25 7	963
40 45	17 88	994
27 70	10 96	1022
11 15	3 87	1059
0	0	1083

The initial freezing points of the gold copper alloys are easy to determine, but the subsidiary or eutectic points are very difficult to detect even if a sensitive autographic recorder is employed We have great confidence in our conclusion that the alloy containing about 82 per cent of gold and 18 of copper, and not the one which contains 55 per cent. of gold, is really the eutectic. The reason, apart from micrographic evidence, is not only that the freezing point of the 82 per cent. gold alloy is lower than that of any other member of the series, but the autographic record reproduced in fig. 2 shows that the angles at A and B, where the solidification begins and ends, are quite sharp, while the portion between A and B, which represents the actual solidification of the alloy, is horizontal Neither of these conditions are met with in the autographic records of the other alloys of the series. Moreover, the fracture of the 82 per cent. alloy is conchoidal, as in the case of a great number of other eutectics, owing to the extremely fine state of division of the constituents, which makes these alloys appear to be homogeneous. The exact composition of the eutectic is, however, difficult to determine

FIG. 2.



A comparison of the freezing-point curve of the gold-copper with that of silver-copper alloys shows that there are striking similarities when the number of atoms in the alloys are taken as abscissæ. It was shown by Levol* as long ago as 1852 that the only homogeneous alloy of silver and copper corresponded in composition with the formula Ag_3Cu_2, and Heycock and Neville† confirmed the anticipation of one of us,‡ which was not verified at the time, that it would prove to be the eutectic of the series. In the gold-copper series the alloy containing 59·49 atoms of gold and 40·41 atoms of copper has a lower freezing point than any other alloy examined, although it is hardly to be distinguished from the alloys containing a little more copper. The curve of fusibility of the series is much more rounded near this point than that of most binary alloys, and bears a superficial resemblance to that of two substances forming a continuous series of mixed crystals, but micrographic study of the series conclusively shows that it possesses a eutectic.

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* Levol, 'Annales de Chim. et de Phys.,' vol 36 (1852), p. 193; vol. 39 (1853), p. 163.

† 'Phil. Trans.,' A, vol 189 (1897), p. 25.

‡ Roberts-Austen, *loc. cit.*

It has been shown by Osmond,* moreover, that silver and copper are each capable of holding a small percentage of the other in solid solution, but that if both metals are present in considerable amounts, the two solidified solutions exist side by side It is evident therefore that they form an interrupted series of "mixed crystals, and that the substance first solidified in cooling a solution of one metal in the other is not a pure metal, but an isomorphous mixture of the two metals containing only a small percentage of one of them This conclusion agrees well with the general shape of the curve of fusibility of the silver copper series, and the still greater concavity of the curve of fusibility of the gold copper series suggested that a similar condition of things is here met with, but that the gap in the series of mixed crystals is much smaller and that the mutual solubility of these two metals is greater

Microscopic examination of the alloys of gold and copper affords evidence that this is really the case but appears to point to the conclusion that more copper can be dissolved in gold than gold in copper Alloys containing only a small percentage of copper consist of large crystals similar in shape to those seen in pure gold and showing no signs of cement between them They differ from those of pure gold in their colour which is reddish or reddish brown, after treatment with nitrohydrochloric acid When magnified 1580 diameters these crystals show a minutely granular structure which resembles that of pure gold and affords no evidence of separation into two constituents Even in standard gold containing only 91 6 per cent of gold the structure is nearly the same and is not unlike that of the ground mass of standard silver containing 92 5 per cent and 7 5 per cent of copper prepared in a similar way On the other hand, the alloys containing less gold than the eutectic show crystals of copper set in a matrix which consists apparently of the eutectic

The following examples of photomicrographs of the series are shown in Plate 1 —

Fig 1 Plate 1, represents the characteristic surface of a small ingot of standard gold The structure was not developed by etching, and the magnification is only 4 5 diameters

Fig 2 is a polished section of standard gold etched by immersion for about 15 seconds in a boiling mixture of equal parts of nitric and hydrochloric acid The magnification is as in the case of No 1 4 5 diameters, and the structure consists of sections cut in various directions by a plane passing through the crystals, of which the mass is composed Fig 3 is the eutectic of the gold copper series, it contains 80 per cent of gold and 20 per cent of copper etched as in the case of the alloy shown in fig 2, the magnification is, however, 1580 diameters, which reveals the banded structure characteristic of a eutectic alloy.

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* Bull. de la Soc d Encouragement,' 5th Series, vol 2 (1897) p 837

Fig. 4 is standard gold etched as before and magnified 1580 diameters.

Fig. 5 is a section etched as before of an alloy containing 27 per cent. of gold and 73 per cent. of copper. In it the presence of two distinct constituents can be seen. The darker portion which has been readily attacked by the acid is copper and the lighter is mainly the eutectic. This fact is proved by fig. 6, which is a very high magnification (6300 diameters) of the lighter portion of fig. 5, and this on close examination reveals the presence of the laminated or banded eutectic.

Another resemblance between the series of gold-copper and silver-copper alloys is to be found in their relative tensile strengths. In both cases the eutectic alloys are extremely brittle and have a lower tenacity than the other members of the series. In the case of gold one of us* has shown that the tenacity of an unworked cast bar of pure gold 7.5 mm. wide and 5.2 mm. thick is 7 tons per square inch, the metal elongating 30.8 per cent. before rupture. Both tenacity and extensibility are greatly increased by the first additions of copper, the tenacity rising in the case of standard gold which contains 8.3 per cent. of copper to more than twice that of pure gold. Under similar conditions, however, we have found that the eutectic alloy of gold and copper has a tensile strength of only 7.87 tons per square inch with an elongation of only 3.3 per cent. It is in fact about as brittle as pure gold alloyed with 0.24 per cent. of lead, which has an elongation of 4.9 per cent. We also determined the extensibility of the eutectic alloy of silver and copper to be only 2.2 per cent., and its tensile strength 29.1 tons per square inch. These are the first cases observed in which eutectic alloys appear to show less tenacity and extensibility than the other members of the series to which they belong. The eutectics of lead and tin, of copper and tin, and of iron and carbon are in each case the strongest alloys of the series, and are not at all brittle. The eutectic of the copper-zinc series is more extensible than any other member of its series, while its tenacity is considerable. The gold-copper and silver-copper alloys differ therefore from other alloys, which appear to be brittle and of low tensile strength only if they have passed through a pasty stage in solidifying, and possess two freezing points, the lower of which is that of the eutectic.

It is clear, from the results given above, that gold and copper cannot be expected to form a series of alloys of uniform composition, but will show evidence of liquation similar to that exhibited by silver and copper, though in a less degree. Much evidence on this point was obtained in the course of the preparation of the standard gold trial

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* Roberts-Austen, 'Phil. Trans.,' A, vol. 179 (1888), p. 339.

Fig 1 Surface of an Ingot of Standard Gold (unetched) x 4 5D

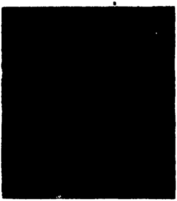

Fig 2 Standard Gold x 4 5

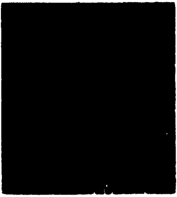

Fig 3. Eutectic (80 per cent Gold 20 per cent Copper) x 1580D

Fig 4 Standard Gold x 1580D

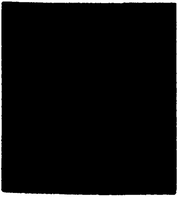

Fig 5 27 per cent Gold, 73 per cent Copper. x 4 5D

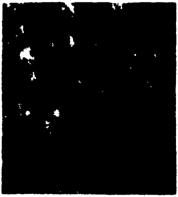

Fig 6 Eutectic portion of 27 per cent Gold, 73 per cent Copper x 6320D

plate, which contains 91·6 per cent. of gold and 8·3 per cent. of copper. This alloy was cast into flat bars of various dimensions, and assays were made on pieces cut from all parts of the plates into which the bars were rolled, care being taken to adopt all the precautions described by one of us with a view to ensuring accuracy in the determinations.* This enabled the limit of error to be reduced to 0·02 per 1000 on a mean of three assays. In all, nine plates were prepared before one of the necessary accuracy was obtained, and over 900 determinations of the proportion of gold present in the assay pieces were made. It was found that in general there was a tendency for the outside of the ingots to be richer in gold than the interior, but that this distribution was hardly so regular, and was not so pronounced as that observed in a contrary sense, in standard silver, in which case silver accumulates in the centre of the mass.

It may be added that the differences in composition of different parts of the gold bars, though small, are many times larger than the possible errors of assay.

The plates were of various dimensions, and were prepared from pure gold and electrodeposited copper, well stirred to ensure uniformity while in the molten condition, cast in iron moulds coated with carbon, and rolled out to a width of about 17·5 cm., a thickness of 1 mm., and a length of from 1 to 1·25 metres, the weight being from 3·7 to 4·6 kilos. Series of discs were then cut out in parallel lines, one down the centre of the plate, and two others distant 1 cm. from the edges. In the case of plate No. 1, intermediate series of discs were cut half way between the centre line and the edges. The means of all assays of each series taken from three typical plates were as follows, each result giving the mean of from 21 to 27 assays :—

	No. 1.	No. 2.	No. 3.
Left side	916·59	916·65	917·08
Left intermediate	0·65		
Centre line	0·49	916·55	916·71
Right intermediate	0·66		
Right side	0·61	916·58	916·95
Mean of all assays	916·589	916·598	916·893
Greatest differences from mean (per 1000 parts)	+0·35	+0·14	+0·60
	−0·30	−0·18	−0·40
Difference between richest and poorest parts of the plate (per 1000 parts)	0·71	0·27	1·00

In the case of standard silver plates of the same size prepared in a similar way, the difference between the amounts of silver in the richest and poorest parts of the plate is usually from 1·0 to 3·0 parts per thousand, or three or four times as great as that in the case of standard gold. The poorest part in the gold plate is, however, always in the centre and the richest part at the outside.

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* Rose, 'Chem. Soc. Journ.,' vol. 63 (1893), p. 704.

The assays made on the "get" of the gold plates, the place on the top of the casting where shrinkage of the mass on solidifying is marked externally by a depression, showed that this part was usually richer in gold than any other part of the plate. These assays are not included in the means given above.

Conclusion.

It will be evident from the results given above, that when a small proportion of copper is added to gold, the alloy sets as a whole, and forms a solid solution. If small amounts of copper are successively added, the limit of solubility of that metal in gold is at length reached, and a eutectic separates, which forms the whole mass when about 82 per cent. of gold and 18 per cent. of copper are present.

Comparatively small additions of gold to copper saturate the latter, and the eutectic makes its appearance before the proportion of gold reaches 27 per cent. The composition of the eutectic corresponds approximately to 60 atoms of gold and 40 of copper, while the silver-copper eutectic also contains nearly 60 atoms of silver and 40 of copper. In other respects also, in the brittleness of the eutectic, in the limited mutual solubility of the two metals, and in the liquation which attends solidification, the gold-copper and silver-copper series resemble each other closely The main difference is that copper appears to be more soluble in gold than in silver, so that the characteristics of the gold-copper alloys are less marked, and consequently have been less easy to detect.

"The Crystalline Structure of Metals." Second Paper. By J. A. Ewing, F.R.S., Professor of Mechanism and Applied Mechanics in the University of Cambridge, and Walter Rosenhain, B.A., St. John's College, Cambridge, 1851 Exhibition Research Scholar, Melbourne University. Received May 17,—Read May 31, 1900.

(Abstract.)

The investigations described in this paper deal principally with the phenomena of annealing The first section of the paper describes experiments made in the hope of observing under the microscope the process of recrystallisation in strained iron. It is well known that

rearrangement of the crystalline structure of iron occurs when the metal is heated to redness, and it is believed that such changes are associated with the evolutions of heat which are indicated by "arrest points" during the cooling of iron. We hoped that by keeping a polished and etched surface of the strained metal under microscopic observation while the specimen was gradually heated, we should see a more or less sudden change in the crystalline pattern at a temperature corresponding to one of the "arrest points." This attempt, however, to watch the process of recrystallisation failed, although the experimental difficulties of keeping a specimen under microscopic observation while it was being heated were successfully overcome. The specimen was electrically heated in a vessel with a thin glass or mica window, and the microscope-objective was kept cool by directing a strong blast of cold air on it and on the surface of the window. In one set of experiments the specimen was kept in an atmosphere of pure hydrogen during the heating, but it was found to become so much tarnished as to obliterate the crystalline pattern. At a red heat, however, the uniform luminous surface of the specimen was seen to develop a number of dark patches which, on slightly raising the temperature, spread over the entire field. No corresponding change was visible during cooling, but the phenomenon would recur every time the specimen was heated, provided it had been cooled below redness after the previous heating. This phenomenon was absent when the specimen was heated in a vacuum, and we believe that it indicates a chemical action between hydrogen and iron, possibly corresponding to the hydrogen arrest point discovered by Sir W. Roberts Austen.*

In the next series of experiments the specimen was heated in a vacuum. On prolonged heating the specimen still became tarnished, but at first the crystalline pattern remained visible up to a bright red heat. No change in the pattern was observed, but subsequent polishing and etching of the same surface showed that a real change of crystalline structure had occurred. The original etched pattern on the surface had persisted after heating, simply because the differences of level and surface texture on which it depended had in no way been disturbed by the recrystallisation. Any crystalline pattern seen under the microscope, whether it be produced by etching or relief polishing, consists either of coloured surface deposits, or of steps, or pits, or other differences of level in the surface, and these differences of superficial texture, like mechanical scratches, are not affected by rearrangement of the crystalline elements. Coloured surface deposits would also remain unaffected. All attempts to observe the actual process of recrystallisation must therefore be unsuccessful.

The next section of the paper deals with the changes of crystalline structure which go on in lead and other metals at comparatively low temperatures. Our attention was directed to this by noticing that a piece of plumber's sheet lead, when etched with dilute nitric acid, exhibits a strikingly crystalline structure, with large crystals. The character of this appearance led us to the view that a slow process of annealing or recrystallisation was at work in such lead at ordinary atmospheric temperatures, and we have satisfied ourselves that this is the case. The method of investigation consisted in taking a series of micro-photographs, at low magnifications, of certain marked areas in the surface of a specimen, in order to watch the change which went on through lapse of time, or after application of some thermal treatment. It was necessary, for the reasons given above, to re-etch the surface before each photograph was taken.

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* Alloys Research Committee Report, 'Proc. Inst. Mech. Eng.,' 1899.

We have observed that when a piece of cast lead is severely strained by compression, the originally large crystals, after being considerably flattened, are driven into and through one another, so that the etched surface of a strained specimen presents a fine grain, whose crystalline nature only becomes apparent under considerable magnification (80 to 100 diameters). A piece of lead severely strained in this way, and kept for nearly six months in an ordinary room without any special thermal treatment, was found to be undergoing continuous change during that time. A series of photographs of this specimen, taken at intervals during the six months, show that a great number of the small crystals have grown larger at the expense of their neighbours. In similar specimens which have been kept at 200° C., the growth has been much more rapid and more pronounced. The rate of growth is a function of time and temperature, but some specimens show much more rapid changes than others under similar conditions of temperature; in some cases five minutes' exposure to a temperature of 200° C. is sufficient to alter the crystalline pattern completely. Experiments have also been made at 100° C. and 150° C., leading to the general result that crystalline growth will occur at any temperature from that of an ordinary room, *i.e.*, 15° C. or 20° C. up to the melting point of lead, and that in general the higher the temperature the more rapid is the initial rate of change. No numerical data can be given, as the crystals are quite irregular, both in size and shape. So far as our observations go, they lead to the result that when such crystalline growth has continued for some time at a given temperature, the structure becomes more or less stable, so far as that temperature is concerned, but exposure to a higher temperature may cause further growth to occur.

A comparison of micro-photographs of the same specimen at various stages reveals the fact that the growth of an individual crystal occurs, not in uniform layers all round it, but by the formation of arms and branches that invade the neighbouring crystals, the intervening portions sometimes changing at a later stage. This action is analogous to the formation of skeleton crystals in a metal during solidification from the liquid state the space between the branches filling in as solidification proceeds

A marked feature observed in several specimens was the large and rapid growth of one or two individual crystals in several instances such individuals grew until they were some hundreds of times larger than their neighbours We have not been able to discover the determining cause of such growth nor in general why one crystal should grow at the expense of its neighbour Generally the most aggressive crystals were found near the edges of the specimen It is noticeable that at times a crystal which has already grown considerably is swallowed up by a more powerful neighbour

Some light is thrown on the nature of these actions by the fact that this growth only occurs in crystals that have been subjected to severe plastic strain By casting the metal in a chill mould specimens of lead can be obtained having a crystalline structure quite as minute as that found in a severely strained specimen but this structure remains unchanged at temperatures which produce rapid change in a strained specimen

The investigation of the effects of such comparatively moderate temperatures was extended to other metals viz tin zinc and cadmium In tin the various phenomena of crystallisation from the fluid state are strikingly illustrated on a large scale by the thin layer of that metal which constitutes the surface of commercial tin plate The effects of rapid and slow solidification in producing small or large crystals respectively are well marked, and in examination of the etched surface of tin plate under the microscope reveals beautiful geometrical markings or pits whose oriented facets produce the well known selective effect of oblique illumination The study of the crystalline structure affords an explanation of the nature and method of production of patterns in moiree metallique a process which has long been in use for the decoration of articles manufactured of tin plate

In tin, also, we find that the smallest structure obtainable by quenching the melted metal in water remains unchanged at all temperatures up to the melting point, on the other hand specimens whose crystalline structure has been modified by great plastic strain exhibit phenomena of recrystallisation at lower temperatures similar to those observed in lead In a piece of strained tin, an hour's exposure to 150 C produced complete recrystallisation Exposure to lower temperatures for this short time produced no visible change, but we have not investigated gradual time effects in this metal The behaviour of strained zinc and cadmium is analogous to that of tin and lead Exposure to 200° C is sufficient to produce rapid recrystallisation in both zinc and cadmium This is particularly marked in the case of ordinary sheet zinc On etching commercial sheet zinc, no large crystals are visible. In this state the metal is strong and tough, bending quite noiselessly. After exposure to 200° C for half an hour, it shows on etching a large brilliant structure, and the metal is then weak and brittle, and when bent, breaks along well marked cleavage planes and emits a "cry" like that of tin.

In cadmium the recrystallisation is comparatively slow at 200° C, and a time effect has been observed; the action is rather different from that observed in lead. In cadmium the size to which the crystals grow appears to be much more uniform; no arms or branches are thrown out and no twin lamellæ are found.

The final section of the paper deals with an hypothesis which is advanced as an attempt to explain the mechanism of the growth of crystals in apparently solid metal.* According to this hypothesis, the metallic impurities which are present in a metal, play an important part in the action. When a metal solidifies from the fluid state the metallic impurities ultimately crystallise as a film of eutectic alloy in the intercrystalline junctions; when fairly large quantities of such eutectics are present the microscope reveals their presence as an intercrystalline cement, such as that formed by "pearlite" in slowly cooled mild steel. Very minute quantities of eutectic, however, will be invisible and yet capable of forming a thin film of fusible cement. We conceive that the changes of crystalline structure which go on while the piece is in the solid state are accomplished by the agency of eutectic films between the crystals in dissolving metal from the surfaces of some crystals and depositing it on others. When a metal is severely strained these films of eutectic will be also strained and in many places broken, thus allowing the actual crystals to come into contact with one another. The difference in the rate of etching of adjacent crystals and the phenomena of the electrolytic transfer, in an acid solution, of lead from one crystal to another in the same mass of metal, support the supposition that there is a difference of electric potential between the crystal faces which are brought into contact by severe strain. If it be assumed that a film of eutectic alloy when fluid, or even when in the pasty condition that precedes fusion, can act as an electrolyte, we may regard any two crystals thus in contact, with a film of eutectic interposed in places, as a very low resistance circuit, and the growth of the positive crystal at the expense of the negative would result. Moreover, such growth would be more rapid at higher temperatures, and its rate at a given temperature would vary in different specimens according to the nature and quantity of the impurities present. That an alloy can act as an electrolyte has not been established experimentally, but the assumption is supported by the close general analogy between alloys and salt solutions. This analogy extends to the very question of the growth of crystals, as

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* It is proper to say that this hypothesis is due to Mr Rosenhain.—J A E

Joly has shown that when crystals of a salt are immersed in their mother-liquor, growth of one at the expense of others will take place.

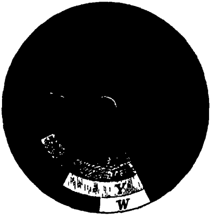
It should be added that solution of one crystal into the intervening film of eutectic, along with deposit on the neighbouring crystal from the eutectic, may occur as a consequence of differences of orientation, producing differences of "solution pressure" apart from actual electrolysis, but the fact that growth has not been observed to occur except in strained crystals favours the view that the action is electrolytic.

Some further results which have been deduced from the above hypothesis have been verified by experiment. It follows from the hypothesis that an inter-crystalline boundary containing no eutectic would be an impassable barrier to crystalline growth, but if the eutectic could in any way be supplied, growth across the boundary might take place. In an absolutely pure specimen of lead, there would be no eutectic at the inter-crystalline junctions, but as extremely minute traces of impurity would suffice to set up the action, it is almost hopeless to verify the hypothesis in this way. Some experiments on the cold welding of lead have, however, borne out our conclusions. Two clean, freshly scraped lead surfaces will unite under great pressure in the cold state, and if a piece so welded be annealed, we find that the crystalline growth due to the annealing, with very rare exceptions, never crosses the inter-crystalline boundary formed by the welding surface. To test whether the presence of some eutectic would allow growth to take place, we have scattered small quantities of a more fusible metal over the freshly scraped surfaces of lead before squeezing them together. Then, after a cold weld had been made by pressure, on annealing by exposure to 200° C. it was found that crystal growths frequently crossed the line of the weld, as the above theory led us to expect. This experiment has been repeated many times with the uniform result that whenever a small quantity of eutectic, or of an impurity capable of forming a eutectic with the lead, was scattered over the clean surfaces before welding, a distinct growth of crystals across the boundary took place as a result of annealing. On the other hand, a large number of welds were made without introducing any impurity, and with very rare exceptions they showed no growth across the boundary, even after the annealing process was continued for some weeks. In rare exceptions a minute amount of growth across the boundary was observed, but these may fairly be accounted for by the almost unavoidable presence of traces of impurity. The result as a whole goes far to confirm this solution theory of crystalline growth in annealing.

"On the Estimation of the Luminosity of Coloured Surfaces used for Colour Discs." By Sir WILLIAM DE W. ABNEY, K.C.B., F.R.S. Received May 5,—Read May 31, 1900.

When a source of light is small, such as the points of an arc light, a candle, or lamp, it is comparatively easy to find the luminosity of any coloured surface which is illuminated by it, using the method which has been described in "Colour Photometry, Part II";* but when the source of light is a large surface, such as the sky, the method therein described is much more difficult to apply. Quite recently, when examining the question of providing suitable screens for producing the negatives required for three-colour photographic prints, it became necessary to devise a plan by which rings of different colours could be made of equal luminosity in ordinary daylight by rotating them with the proper proportions of black. The rings were concentric and rotated as a disc, see fig. 1, and the difficulty encountered was to ascertain what amount of black ought to form part of each ring.

FIG. 1.



S is the nut of the spindle.
V is a violet disc (methyl violet).
B is a portion of a blue ring (French ultramarine).
R „ „ red ring (vermilion).
G „ „ green ring (emerald green).
Y „ „ yellow ring (chrome yellow).
W „ „ white ring.

In "Colour Photometry, Part III,"† it was shown that only one ray of the spectrum a greenish yellow, progressed in luminosity at the same rate as white light Thus if part of a white screen were illuminated by this colour and another part by white light and the luminosities were equal (say) to one candle then if the two beams were equally diminished they would still match in luminosity until the light was so feeble that it ceased to stimulate the retina Other rays lying not far from this ray, both on the red and green side of it give practically the same results When however the red was compared with the white each being made equal (say) to one candle equal diminution of the beams did not show the luminosities as the same the red becoming rapidly less luminous than the white With the blue green the blue and the violet the reverse was the case the white becoming darker than the colour as the beams were equally diminished

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* Abney and Festing, 'Phil. Trans.,' A, 1888.

† Abney and Festing, 'Phil. Trans.,' A, 1892.

A more extended research which is nearly complete shows that the observations recorded in Part III of Colour Photometry are correct and can be applied to the problem which I wished to solve

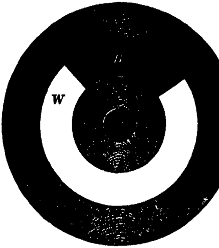
Further it was shown in the same paper that colour disappeared from all rays of the spectrum long before (except in the case of the pure red) their light was extinguished this last owing to the feeble stimulation of the retina Naturally, as the colour began to disappear, the matching of the luminosity of the ray under consideration with that of white became easier to carry out

These facts made it possible to devise a ready method to ascertain the luminosity of any colour If we take two yellow discs, one (say) 8 inches in diameter and the other 4 inches and between them sandwich a pair of interlaced black and white discs of 6 inches diameter, and rotate the four discs on a rotating machine at a speed which will make the black and white into a grey without scintillation, this grey can be made, by altering the proportion of black to white to match the luminosity of the yellow A very exact match can be obtained by observing the discs through a black transparent medium, such as the black obtained on a photographic plate after development with methol or amidol developers The deposit may be so dense that the yellow colour may practically disappear and the two dull greys may then be readily matched The luminosity of the yellow in terms of the white is given by the angle which the white subtends when the small proportion of white reflected from the black annulus is added to it

The same procedure may be adopted for a green colour and its luminosity be obtained It may be stated that four or five observations for each colour should be made if great exactness is required

When the luminosities of these two colours have been determined, 4 inch discs of them may be interlaced with a blue, and a grey formed, which can be matched with a grey formed of black and white as before

FIG. 2.



YY are yellow discs.
B^k is a black disc
W ,, white disc.
S is the nut of the spindle.

From the angles which the sectors of the colours subtend and of the black and white employed, the luminosity of the blue can be calculated. The luminosity of the blue being ascertained, a red disc may be interlaced with the green and the blue disc, and that of the red calculated. As a check a black and yellow disc may be interlaced and compared with the colour given with the red and green discs interlaced, one of the pairs of course being of greater diameter than the other.

To ascertain what degree of accuracy could be attained the following experiment is given in detail. The light used was the arc light, and the measurements as described above made.

It was found that the black reflected 3·33 per cent. of white light, and that when the luminosity of the yellow was matched the interlaced black and white discs occupied 82° and 278° respectively of the compound disc. This gave the yellow a luminosity of 78, white being 100. In a similar way the luminosity of emerald green was found to be 43. These two discs were interlaced with a dark blue disc and a grey formed which matched a grey formed by black and white. The following equation was obtained:—

$$\begin{array}{ccccccccccc} \text{Yellow.} & & \text{Green.} & & \text{Blue.} & & \text{White.} & & \text{Black.} & & \text{White.} \\ 118 & + & 71 & + & 171 & = & 122 & + & 238 & = & 130 \end{array}$$

Yellow

The luminosity of 118 = $\frac{118}{360}$ of 78 = 25 6

Green

" 71 = $\frac{71}{360}$ of 43 = 8 5

White

" " 130 = $\frac{130}{360}$ of 100 = 36 1

Blue

The luminosity of 171 is therefore represented by

$$36\ 1-(25\ 6+8\ 5)=2$$

The luminosity of the blue pigment is therefore

$$\frac{360}{171}\text{ of }2=4\ 2$$

The luminosities of the three pigments were then compared with white by the method described in Part II of ' Colour Photometry," and found to be

Yellow	77 7
Green	43 2
Blue	4 1

The luminosity of the blue only differs by that found by the new plan by 0 1, which is a very close approximation

The red disc was then interlaced with the blue and the green, and a grey formed as before, and from calculation it was found that it had a luminosity of 32 5 Measuring it by the old plan, the luminosity came out as 32 7

Having obtained the luminosity of the three standard colours, that of any other colour can be calculated by substituting for one of them a disc of such colour and again making a grey and matching it with a grey formed by the black and white It will be noticed that this method can be carried out in any light, whether candle light, electric light, or day light, but of course the luminosities of the colours will vary according to the quality and kind of light employed

When the luminosities of the colours are determined, the angles which the segments of the annuluses in fig 1 should subtend can be calculated after taking into account the luminosity of the black employed

When the disc is rotated round S, each colour should be equally luminous, and if by means of an appropriate screen, placed in front of the lens, the image of the disc impresses the photographic plate in such o make the density of each part of the negative the same on development, then all objects photographed with such a screen interposed on similar plates will be rendered in proper gradations of light and shade regardless of their colour or colours.

"The Diffusion of Ions produced in Air by the Action of a Radio-active Substance, Ultra-violet Light, and Point Discharges."

By JOHN S. TOWNSEND, M.A., Clerk Maxwell Student, Cavendish Laboratory, Fellow of Trinity College, Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received May 17,—Read June 14, 1900.

(Abstract.)

The researches described in this paper form a continuation of those published on the Diffusion of Ions into Gases.* The latter paper gives the results of experiments made with ions produced in air, oxygen, hydrogen, and carbonic acid by the action of Röntgen rays. The gases in these experiments were at atmospheric pressure.

The present paper contains similar investigations for ions produced in air at various pressures by the action of a radio-active substance, and also determinations of the rate of diffusion of ions produced in air at atmospheric pressure by the action of ultra-violet light and point discharges.

The principle of the method consists in calculating the coefficient of diffusion from observations on the loss of conductivity of a gas as it passes along metal tubing. The experiments were arranged so that in all cases the loss of conductivity due to diffusion should be much greater than the loss due to other causes, so that it was not necessary to apply any corrections for losses arising from recombination or from the mutual repulsion of the ions.

The results of the experiments are given in the following tables. Tables I, II, III, and IV give the coefficients of diffusion, K, of positive and negative ions in dry and moist air at various pressures, P, the ionization being produced by the action of a radio-active substance. The temperature of the air during each experiment is given in the column Θ.

These tables show that in each case the rate of diffusion of ions into a gas is inversely proportional to the pressure of the gas.

The coefficients of diffusion at 772 mm. show a discrepancy from this law, which is somewhat greater than the probable error of the experiments, but we should not expect a closer agreement between the products P × K unless the temperature of the air was the same in

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* 'Phil. Trans.,' A, vol. 193, pp. 129—158.

Table I.—Positive Ions in Dry Air.

P	K.	P × K	θ.
772	0·0317	24·5	19
550	0·0420	23·1	13
400	0·0578	23·1	16
300	0·078	23·4	13
200	0·118	23·6	12

Table II.—Negative Ions in Dry Air.

P.	K.	P × K.	θ.
772	0·0429	33·0	19
550	0·0542	29·8	13
400	0·078	31·2	16
300	0·103	30·9	13
200	0·155	31·0	12

Table III.—Positive Ions in Moist Air.

P.	K.	P × K	θ.
772	0·0364	28·0	18
400	0·0668	26·7	11
200	0·134	26·8	9·5

Table IV.—Negative Ions in Moist Air.

P.	K.	P × K.	θ.
772	0·0409	31·5	18
400	0·0771	30·8	11
200	0·0147	29·4	9·5

The values of K are expressed in $\frac{(\text{centimetre})^2}{\text{second}}$.

The values of P are expressed in millimetres of mercury.

each case. It will be noticed that the experiments at 772 mm. were made when the temperature of the air was higher than the temperatures during the other experiments.

The negative ions which are produced when ultra-violet light falls on a zinc plate diffuse into air at nearly the same rate as the negative ions produced by a radio-active substance. The values of the coefficients of diffusion for dry and moist air are 0·0435 and 0·0375 respectively, the pressure being 760 mm. and temperature 17° C. in each case.

The rates of diffusion of ions produced by a point discharge were found to vary considerably. The discharges were usually produced from a steel needle or platinum wire pointing along the axis of a metal tube. It was found that when the point was at the open end of the tube the ions which were produced diffused more rapidly than those given off when the needle, or wire, was drawn back into the tube, so that the point should be a few centimetres from the open end. The differences obtained in this way were greater when the air was dry than when it was saturated with moisture. The following are the limits between which the coefficients of diffusion of ions, produced by point discharges, were found to vary:—

Positive ions in dry air	0·0247—0·0216
Negative ions in dry air..........	0·037 —0·032
Positive ions in moist air	0·028 —0·027
Negative ions in moist air ...	0·039 —0·037

"Static Diffusion of Gases and Liquids in Relation to the Assimilation of Carbon and Translocation in Plants."* By HORACE T. BROWN, F.R.S., LL.D., and F. ESCOMBE, B.Sc., F.L.S. Received May 31,—Read June 14, 1900.

(Abstract.)

This paper is intended to be the first of a series descriptive of the work carried out by the authors in the Jodrell laboratory on the fixation of carbon by green plants, and deals mainly with the purely physical processes by which atmospheric carbon dioxide gains access to the active centres of assimilation.

The new evidence which F. F. Blackman brought forward in 1895 in favour of the gaseous exchanges of leaves taking place exclusively through the stomatic openings, presents at first sight certain difficulties of a physical nature, which have led to an examination of the whole question of the free diffusion of carbon dioxide at very low tension, and under a set of conditions very different from those under which the previous determinations of the coefficient of diffusion of carbon dioxide and air have been made by Loschmidt and others, where the gases were initially of equal tension, and the ratios of mixture departed widely from those of ordinary atmospheric air. The inquiry has led to the discovery of some new facts connected with the static diffusion of gases and liquids, which are of considerable interest, not only from the physical point of view, but from the explanations they suggest of certain natural processes which are primarily dependent on diffusivity.

The method employed in the first instance for the determination of the diffusivity of atmospheric carbon dioxide was one of *static diffusion* down a column of air of a definite length, towards an absorptive surface at the bottom of the column. When a static condition has been established, there is a steady flux of the carbon dioxide down the

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* The title of the paper as communicated to the Society was "Some New Observations on the Static Diffusion of Gases and Liquids, and their Significance in certain Natural Processes occurring in Plants."

air column which may be quantitatively investigated by the same simple mathematical treatment as the "flow" of heat in a bar when the permanent state has been reached, or the "flow" of electricity between any two regions of a conductor maintained at a constant difference of potential

By a long series of experiments of this nature it was found that the diffusivity constant, *k*, for very dilute CO does not materially depart from the value assigned to it by Loschmidt and others, when experimenting with much higher ratios of mixture, and that the difference is certainly not of sufficient magnitude to be taken into serious account in the study of the natural processes of gaseous exchange in the assimilating organs of plants

In the static diffusion of a gas, vapour, or solute, as the case may be, the amount of substance diffusing in a given time, all other conditions being the same, is directly proportional to the sectional area of the column It is found, however, that if the flow is partially obstructed by interposing at any point in the line of flow a thin septum pierced with a circular aperture, the rate of flow across unit area of the aperture is greater than it would be across an equal area of the unobstructed cross section of the column at this point If the margin around the aperture has a width of at least three or four times its diameter, the rate of flow is now found to be directly proportional to the *linear dimensions* of the aperture and not to its area, so that the velocity of flow through unit area varies inversely as the diameter

A large number of experiments on the diffusion of carbon dioxide, water vapour, and sodium chloride in solution, are given in support of this proposition All these show that the rate of diffusion across such a septum, all other conditions being the same, is directly proportional to the diameter of the aperture, and not, as might have been expected, to its area

Exactly the same result is obtained when small circular discs of an absorbent, such as a solution of caustic alkali, are surrounded by a wide rim and exposed to *perfectly still* air, the amount of carbon dioxide absorbed under these conditions being proportional to the *diameters* of the discs

If, however, there are any sensible air currents the absorption becomes proportional to the areas

These two sets of phenomena may be explained as follows —

In the case of the absorbing disc in perfectly still air, the convergent streams of carbon dioxide creep through the air towards the absorbing disc, establishing a steady gradient of density, and this creep will be a flux perpendicular to the lines of equal density, which form curved surfaces or "shells" surrounding the disc and terminating in the rim. The state of things is exactly analogous to the electric field in the neighbourhood of a conductor of the same shape and dimensions as the absorbent disc * In the case of the gas, the curves or "shells" of equal density are the analogues of the similarly curved surfaces of equipotential above the electrified disc, whilst the converging lines of creep or flux of the gas are the analogues of the lines or tubes of force which bend round into the disc as they approach it

If we consider two such absorbent discs of different diameters, the curved surfaces in each system corresponding to a given density will be found at actual distances from the discs which are in the same proportion to each other as are the diameters of the discs In other words, the gradient of density on which the rate of flow depends will be proportional to the diameters of the discs, which is exactly what is found experimentally

This case of an absorbent disc is the exact converse of one which has been theoretically investigated by Stefan, viz, the conditions of evaporation of a liquid from a circular surface He found that the lines of flux of the vapour proceeding from the surface of the liquid must be hyperbolas, whilst the curved surfaces of equal pressure of the vapour must form an orthogonal system of ellipsoids, having their foci, like the hyperbolas, in the bounding edges of the disc This was a purely mathematical deduction which has never been verified experimentally, but it will be seen that the exactly converse phenomena of diffusion are in complete agreement with it

In the other case of a diffusive flow through a circular aperture in a diaphragm, the lines of flow, which are *convergent* as they approach the aperture, bend round their foci situated in the edges of the disc and form a *divergent* system on the other side If the chamber into which they pass is a perfectly absorbent one, and is sufficiently large, there will be formed on the inner side of the diaphragm a system of density shells similar to those outside, but with the gradient of density centrifugally instead of centripetally arranged This system of shells is termed negative, and is as effective as the outer positive system in regulating the flow according to the "diameter law," so that this law will still hold good even if the outer air currents are sufficient to sweep away the external positive shells altogether

All the known facts of diffusion through circular apertures in a diaphragm are in complete accord with the above explanation, which is fully elaborated in the original paper

By diffusing colouring matter through apertures in a septum, under such conditions as to prevent convection currents, the "density shells" have been rendered visible, and it has been shown that their ellipsoidal form is exactly that which is demanded by the above hypothesis Moreover, this method gives an experimental demonstration of the more rapid projection of the diffusing particles from the edges of the aperture than from a point nearer its centre, a fact completely in harmony with the deduction of Stefan regarding the evaporation of liquids under analogous conditions

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* The authors are indebted to Dr Larmor for this suggestion of the electrostatic analogy

The various cases which present themselves in practice with regard to the rate of diffusion through single apertures in a diaphragm are then discussed from the above point of view and simple formulæ for the determination of this rate for single and double systems of density shells are established (1) for cases where the thickness of the diaphragm is negligible and (2) for other cases where the apertures become more or less tubular In a subsequent section of the paper it is shown how closely the observed facts conform to these deductions, and that in static diffusion through apertures in a septum we have a new and accurate method for the determination of the diffusivity constants of atmospheric CO_2 of the vapours of liquids, and of substances in a state of solution

Since the velocity of the diffusive flow through unit area of an aperture in a diaphragm varies inversely with the diameter, it might reasonably be expected that a diaphragm could be perforated with a series of very small holes arranged at suitable distances from each other, as to exercise little or no sensible obstruction when it was interposed in a line of diffusive flow, although the aggregate area of the small holes might represent only a small fraction of the total area of the septum Multiperforate diaphragms of this kind were found to possess all the remarkable properties which had been anticipated

The material used for the septa was very thin celluloid, which was perforated at regular intervals with holes of about 0 38 mm in diameter Details of a number of experiments with such diaphragms are given, in which it is shown that they may be so arranged as to produce but little obstructive influence on the diffusive flow of a gas when the total area of the apertures amounts only to about 10 per cent of the area of the septum and that nearly 40 per cent of the full diffusive flow may be maintained when the number of the apertures is so far reduced as to represent an area of only 1 25 per cent of the full area of the septum

The explanation is to be found in the local intensification of the gradient of density in the immediate neighbourhood of the diaphragm, and which does not extend to the column away from the apertures This disturbance of gradient is brought about by the rapid convergence of the lines of flux, and their divergence on the other side, with the consequent formation of a system of ' density shells over each aperture A system of perforations of this kind may be compared with a system of conductors electrified to a common potential, the density of the diffusing substance above the apertures corresponding to electric potential, and the non absorbing portions of the diaphragm to a surface formed by lines of electric force Just as the electric capacity of a plate is not much reduced by cutting most of it away, so also is it possible to block out a large portion of the cross-section of the diffusing column without materially altering the general static conditions on which the flow depends

The importance of these results in relation to diffusion through porous septa is next considered, diffusion through a thin porous septum being only an extreme case of free diffusion through a multi perforate diaphragm, whose apertures are so far reduced in size as to materially interfere with the mass movement of the diffusing substance

A section of the paper is devoted to the application of these new observations to the processes of gaseous and liquid diffusion in living plants, and it is pointed out that the structure of a typical herbaceous leaf illustrates in a striking manner all the physical properties of a multiperforate septum Regarded from this point of view it is shown that the stomatic openings and their adjuncts constitute even a more perfect piece of mechanism than is required for the supply of carbon dioxide for the physiological needs of the plant, and instead of expressing surprise at the comparatively large amount of the gas which an assimilating leaf can take in from the air, we must in future rather wonder that the intake is not greater than it actually is

From data afforded by actual measurements of the various parts of the stomatal apparatus of the sunflower it is shown that an extremely small difference of tension of the carbon dioxide within the leaf, as compared with that in the outer air, will produce a gradient sufficient to account for the observed intake during the most active assimilation

It is also shown that the large amounts of water vapour which pass out of the leaf by transpiration are well within the limits of diffusion, and that it is unnecessary to assume anything like mass movement in the outcoming vapour

The translocation of solid material from cell to cell in the living plant is next considered, especially with reference to this transference, being, at any rate in part, brought about by means of the minute openings in the cell walls through which the connecting threads of protoplasm pass Notwithstanding the very small relative sectional area of these perforations, they probably exercise an important function in cell to cell diffusion, in virtue of their properties as multiperforate septa

There are two appendices to the paper, one in which a full description is given of a series of experiments on the absorption of carbon dioxide by solutions of caustic alkali from air in movement, the second being devoted to a detailed description of the methods used for accurately determining the carbon dioxide absorbed

The Electrical Effects of Light upon Green Leaves (Preliminary Communication)

By AUGUSTUS D WALLER M D, F R S

Received June 6 —Read June 14 1900

In connection with an investigation of electrical effects of light upon the retina,* I have examined vegetable protoplasm (green leaves) with reference to electrical effects that might be expected to occur in connection with the chemical changes produced by light

Under certain favourable conditions that I hope to determine further, a true electrical response to light is obtained, consisting in the establishment of a potential difference between illuminated and non illuminated half of a leaf amounting to 0 02 volt

Among ordinary garden leaves, I have found to be well adapted to demonstration those of young Iris plants about 6 inches high, and of "ten week Stocks in active growth The former, tested by Sachs' method, exhibited no evidence of starch activity in consequence of insolation, the latter in favourable instances exhibited marked deposit limited to illuminated parts Leaves of Tropæolum of Begonia, and of Nicotiana have also proved to be suitable objects of study

Most of the following description refers to young Iris leaves in the first half of the month of May

The method of observation is as follows —

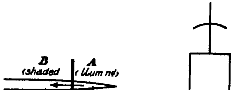

FIG 1 —Normal Response to Light Iris leaf Primary negative ' effect during illumination

A freshly cut but otherwise uninjured leaf is laid upon a glass plate and connected with a recording galvanometer by means of two unpolarisable† electrodes A and B One half of the leaf is shaded by a piece of black paper Leaf and electrodes are enclosed in a box, provided with a shuttered aperture, through which light can be directed A water trough in the path of light serves to cut out more or less heat A glass jar inverted over the leaf and electrodes forms a moist chamber to delay drying During illumination the galvanometer spot is deflected so as to indicate current in the leaf itself from excited part to protected part, i e if B is shaded, light falling upon A arouses current in the leaf from A to B, if A is shaded, light falling upon B arouses current from B to A

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* 'Proc Roy Soc,' March 29; and Phil Trans B 1900 (in the press)

† Du Bois Reymond s electrodes of the usual type, not D Arsonval s which are rendered electromotive by light Illumination of a chloride of silver electrode by a '16 C P" lamp at 10 cm distance gave a reaction amounting to between 0 002 and 0·003 volt A zinc electrode under similar conditions gave a reaction of about 0·0001 volt, in which, however I did not attempt to distinguish separate effects of light and heat

1 The deflection begins and ends sharply with the beginning and end of illumination

2 It is provoked slightly by diffuse daylight, more considerably by an electric arc light, and in greatest degree by bright sunlight

3 It is abolished by boiling the leaf and by the action of anaesthetics

These are the main facts proving that the living leaf responds electrically to the stimulus of light

At this preliminary stage two points of doubt occur to mind and require to be tested, viz , possible effects of heat and of surface evaporation that necessarily accompany illumination

These effects are small in comparison with the true response, and of opposite sign Illumination of a dead leaf gives little or no effect, and what little effect there is, is directed in the leaf towards the illuminated half, where heating and evaporation are provoked

The true response to light varies with varying physiological states of the leaf and of its parent plant

Not every leaf gives response, nor is the response of equal magnitude in different leaves to luminous stimulation (arc light) of constant intensity and duration

The external condition by which the state of leaf is most obviously governed is *temperature*

My first experiments were made upon Iris leaves taken almost at random from young plants (old roots) about 6 inches high at the end of March (temperature not noted, but presumably below 15°) The response to light was between 0 001 and 0 002 volt

The next set of experiments commenced on May 8th on young leaves of similar plants

The responses then observed were

Warm	May 8	0 005
	„ 10	0 008, 0 025
Cold, 10°	„ 11	0 005
	„ 12	nil
	„ 13	nil

I thereafter took note of the external temperature and tested the leaves in a warm box with satisfactory results

A few days later (May 21st) Iris leaves even in the warm box were notably inert Two leaves were tested with negative results a third leaf gave a response of 0 008 volt but its resistance was obstinately high (nearly 3 megohms) a fourth leaf gave a response of 0 004 volt (plates 1770 and 1771) On May 23rd I was unable to find a satisfactory leaf most of the plants were fully grown and in flower I therefore abandoned Iris and sought for other satisfactory leaves, in which it might be possible to obtain evident differences of reaction in correlation with evident differences of state

To sum up the effect of temperature upon the response of Iris—the normal response at 15° to 20° is diminished or abolished at low temperature (10°) augmented at high temperature (30°), diminished at higher temperature (50°), and abolished by boiling

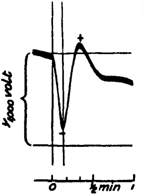

FIG 2a—Normal Response of Nerve to Excitation

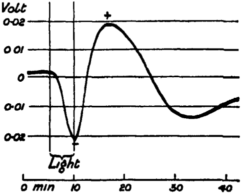

FIG 2b—Normal Response of Iris Leaf to Illumination (1752)

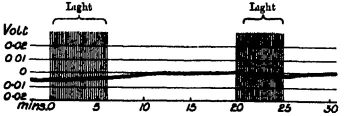

FIG 3—Failure of response in an inert leaf of Iris

Time of day—Leaves of Iris appear to give more marked response at or about mid-day, than at or about 6 P M

Young leaves of young plants act well Old leaves of old plants do not act at all The older leaves of young plants act better than the younger leaves of old plants.

Other plants—Leaves of Tropæolum and of Mathiola, as far as I have yet seen, give a response to light, that is in the main the contrary of the ordinary Iris response, viz, "positive" during illumination, and subsequently "negative"*

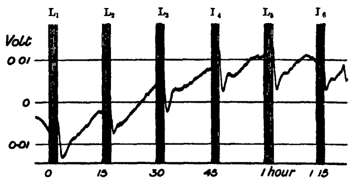

Fig 4—Series of Normal Responses of Mathiola annua ('positive' during illumination, subsequently negative) (1793)

Leaves of Nicotiana reacted like Iris

Leaves of Begonia have given a variety of responses strongly suggestive of the simultaneous action of two opposed forces effecting a resultant deflection in a + or − direction

As regards Mathiola and Tropæolum, leaves empty of starch have acted better than leaves laden with starch

Leaves of Ulva gave no distinct response (only one series of trials)

Leaves of ordinary garden shrubs and trees, &c (e g, Lilac, Pear, Almond, Mulberry, Vine, Ivy), and petals of flowers, gave no distinct response

Anæsthetics—I was able to make only three satisfactory experiments with Iris leaves, before the supply of available material had come to an end †

The first was made upon a vigorous young leaf on May 15th, the test (five minutes' illumination) being made at intervals of rather more than half an hour, with the following result —

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* "Negative" as the term is employed in physiological literature, i e, negative pole of positive element ('zincative)

† *Note added July 16th*—I have made further trial of anæsthetics during the past month upon leaves of Begonia The effect was perfectly clear, but slow—viz, temporary abolition of response by ether vapour, permanent abolition by chloroform vapour, augmentation by little CO_2 temporary suppression by "much" CO_2 I think it possible that the refractory behaviour of the Iris leaf mentioned in the text may have been due to a primary effect of the anæsthetic upon stomatal guard cells (*Vide* 'Proc Physiol Soc,' June 30)

Response before CO_2	− 0·008 volt	
during and after CO_2	= nil	
subsequently	− 0·013	,,
during and after CO_2	= nil	
subsequently	− 0·010	,,

The second experiment was made upon a rather "old" leaf on May 21st, the test being applied at intervals of 40 minutes, and the leaf chamber being at 25°

1	Normal response	0·004 to 0·005
2	After chloroform	− 0·003, 0·002, 0·005
3	After more chloroform	0·005, 0·008
4	After carbon dioxide	0·002, nil, 0·001, 0·012, 0·005

Upon other leaves (Mathiola, Tropæolum) I have witnessed—

1 Augmentation of response in consequence of an air supply containing 1 to 3 per 100 of CO_2

2 Prompt abolition of response when a full stream of CO_2 is run through the leaf chamber

3 Gradual abolition of response when the air supply to the leaf chamber has been kept clear of CO_2, followed by gradual recovery on the readmission of a small amount of CO_2

The action of ether upon a leaf of *Nicotiana Tabacum* was as follows —

Time 0	Normal	0·0016
—	Etherisation	—
15 mins		0·0016
30 ,,		0·0004
45 ,,		− 0·0008
60 ,,		0·0008
90 ,,		0·0016
150 ,,		0·0020

each period of illumination lasting for 2 minutes

Nature of the Normal Response (Iris Leaves)

Direction —The accidental or "normal" leaf currents observed when the electrodes are first applied to a leaf are of no significance, as regards the response to light Such "normal" current may be due to accidental injury or to physiological inequality or to unequal imbibition of contacts, and necessarily includes the small amount of current that may arise from the unpolarisable electrodes It may be positive negative, or non-existent

The regular and normal response to light is independent of such accidental currents provided they be not due to excessive physiological differences

The immediate effect of light is to arouse current in the half shaded leaf directed from the illuminated to the shaded half (*i e* in the galvanometer from shaded to illuminated *i e* from resting to active tissue as in muscle and nerve)

With illumination of moderate duration *i e* not exceeding a few minutes this first effect lasts as long as its cause rising towards a maximum With longer illumination a maximum is reached from which the effect begins to decline The current drops to or beyond zero giving place to the reversed current which is the regular after effect of illumination

At the end of an illumination of moderate duration the current rapidly subsides and gives place to a reversed current directed in the leaf towards the previously illuminated half

This effect and after effect of illumination are similar in appearance to the effect and after effect in nerve produced by tetanisation extending however over longer periods of time (figs 2 and 2*b*)

Magnitude —The electromotive force of the response has a value that usually ranges from 0 005 to 0 020 volt

The leaf resistance (interpolar distance 5 cm and breadth = about 1 cm) is generally between 500 000 and 1 000 000 ohms

The current deflection with these values is between 5 and 40 cm of scale, with a possible accidental effect of ± 1 cm *

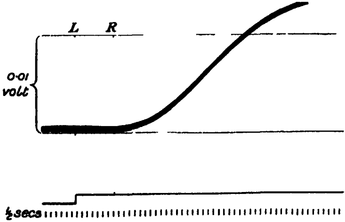

FIG 5 —Interval of Time between Illumination L and Response R of a vigorous Leaf of Iris (1783)

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* The sensitiveness at which this galvanometer was used was such that 10^{-9} A = 1 cm scale With the recording galvanometer 1 cm of ordinate = $3\ 10^{-9}$ A

Summary of Observations on Iris

Date	No			Temperature	Response to light (Volt) Effect	Response to light (Volt) After effect	Resistance (Ohms)
March	1732	1	Sunlight	Cool	−0·0015	+ off scale	—
May 10	1735	2	Arc light	Warm	−0·025	+0·020	710 000
	1736				−0·021	+0·017	740 000
	1737				−0·010	+0·010	740 000
	1738				−0·013	—	710 000
11	1739	3	Sun		−0·008	—	1 000 000
	1740				−0·005	+0·005	590 000
	—		Arc (evening)	Cool	Nil	Nil	—
12	—		Sun and arc	10 to 12	Nil	Nil	—
13	1743	4	Arc	10 to 1[illegible]	0·001 to 0·003		425 000
	1744			1[illegible]	−0·0[illegible]		565 000
	1745		(all subsequent experiments with arc light through water tank)	[illegible]0	0·0[illegible]	—	450 000
14	1746	5	Showing that the response is reversed with rise of temperature	13	−0·004	—	300 000
	"			2[illegible]	−0·0[illegible]0	—	1 [illegible] 000
	"			13	−0·008	+0·006	7[illegible] 000
15	1747			11°	0·004	+0·005	810 000 630 000
	1748			[illegible]5	0·008	+0·008	450 000
				[illegible]5	0·008	+0·010	450 000
	1749	6		[illegible]5	−0·018	—	390 000
				25	−0·01[illegible]	—	39[illegible] 000
	1751			25	−0·013	—	—
16	1752			25	−0·0[illegible]3	+0·017	950 000
	1753		Leaf giving a very well marked and regular response	25	−0·023	+0·028	950 000
	1754		On the second day the resistance is much increased the response is sluggish with a long latent period and no positive after effect at first	25	−0·023	+0·025	950 000
17	1756			12	−0·020	Nil	3,250 000
	1757			25	−0·035	+0·015	2 450 000
	1760			17°	−0·020	+0·020	3 400 000
	1761			16°	−0·030	+0·020	4,250 000

Summary of Observations on Iris—*continued*

Date	No.			Temperature	Response to light (Volt) Effect	Response to light (Volt) After-effect	Resistance (Ohms)
May 19	1764	8	No response	27°	—	—	5 650 000
	1765	9		2	—	—	550 000
	1766			30°	—	—	1 400 000
	1768	10		15°	−0·003	+0·006	560 000
	1769		After immersion in boiling water	15	+0·0003	—	94 000
21	1770	11	Unusually high resistance. Two previous leaves gave no evident response	17°	−0·008	—	2 850 000
	1771	12		25	−0·004	+0·005	500 000
	1772			25	−0·005	+0·010	—
	1773		Chloroform (no effect)	25	−0·003	—	500 000
	1774			25	−0·007	—	250 000
	1775			25	−0·005	+0·005	340 000
	1776		Chloroform (no effect)	25	−0·005	+0·005	370 000
	1777			25	−0·008	—	370 000
	1778		Carbon dioxide (abolition)	25	−0·002		250 000
	1779			25	Nil	—	250 000
	1781		Recovery after CO	25	−0·001 to −0·012	—	250 000
	1782			25	−0·005	—	300 000
			After immersion in boiling water	25	Nil	—	150 000
23	—	13	Sun and arc (no response)	22	Nil	—	—

Latency.—The effects and after-effects occur very sharply at beginning and end of strong illumination of moderate duration. The latent period is between 3 and 10 sec.

Fatigue and Recovery.—The effects of successive illuminations (of 5 minutes' duration) progressively diminish if repeated at 'short' intervals (10 minutes). At intervals of about 1 hour successive illuminations of 5 minutes produce approximately equal effects.

With the leaf of Mathiola I have used periods of illumination of 2 minutes at intervals of 15 minutes without provoking any obvious sign of fatigue.

Conclusion.—The leaves of certain plants under favourable conditions of life exhibit electromotive effects and after-effects amounting to ± 0.02 volt in response to illumination.

As in the case of animal tissue it is possible that the negative (zincative) effect may be significant of dissimilation, and the opposite effect or after-effect significant of assimilation.

The absence of distinct response in petals indicates that chloroplasts are essential to the reaction.

The absence of distinct response in the green leaves of trees and shrubs is possibly due to a lower average metabolism in such leaves, as compared with the activity of leaves of small young plants in which leaf functions are presumably concentrated within a smaller area.

"On the Viscosity of Gases as affected by Temperature." By LORD RAYLEIGH, F.R.S. Received June 20,—Read June 21, 1900.

A former paper* describes the apparatus by which I examined the influence of temperature upon the viscosity of argon and other gases. I have recently had the opportunity of testing in the same way, an interesting sample of gas prepared by Professor Dewar, being the residue, uncondensed by *liquid hydrogen*, from a large quantity collected at the Bath springs. As was to be expected † it consists mainly of helium, as is evidenced by its spectrum when rendered luminous in a vacuum tube. A line, not visible from another helium tube, approximately in the position of D (Neon) is also apparent.‡

The result of the comparison of viscosities at about 100° C. and at the temperature of the room was to show that the temperature effect was the same as for *hydrogen.*

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* 'Roy. Soc. Proc.,' vol. 66 (1900), p. 68.

† 'Roy. Soc. Proc.,' vol. 59 (1896), p. 207; vol. 60 (1896), p. 56.

‡ I speak doubtfully, because to my eye the interval from D_1 to D_3 (helium) appeared about equal to that between D_3 and the line in question, whereas according to the measurements of Ramsay and Travers ('Roy. Soc. Proc.,' vol. 63 (1898), p. 438), the wave lengths are—

In the former paper the results were reduced so as to show to what power (n) of the absolute temperature the viscosity was proportional.

	n	$c.$
Air	0·754	111·3
Oxygen	0·782	128·2
Hydrogen Helium } .. .	0·681	72·2
Argon	0·815	150·2

Since practically only two points on the temperature curve were examined, the numbers obtained were of course of no avail to determine whether or no any power of the temperature was adequate to represent the complete curve. The question of the dependence of viscosity upon temperature has been studied by Sutherland,* on the basis of a theoretical argument which, if not absolutely rigorous, is still entitled to considerable weight. He deduces from a special form of the kinetic theory as the function of temperature to which the viscosity is proportional

$$\frac{\theta^{\frac{1}{2}}}{1+c/\theta} \qquad . \qquad . \quad . \qquad (1),$$

c being some constant proper to the particular gas. The simple law $\theta^{\frac{1}{2}}$, appropriate to "hard spheres," here appears as the limiting form when θ is very great. In this case, the collisions are sensibly uninfluenced by the molecular forces which may act at distances exceeding that of impact. When, on the other hand, the temperature and the molecular velocities are lower, the mutual attraction of molecules which pass near one another increases the number of collisions, much as if the diameter of the spheres was increased. Sutherland finds a

D_1	5895·0
D_2	5889·0
D_3	5875·9
D_4	5849·6,

so that the above-mentioned intervals would be as 19·1 : 26·8. [*June* 23.—Subsequent observations with the aid of a scale showed that the intervals above spoken of were as 20 : 21. According to this the wave-length of the line seen, and supposed to correspond to D_4, would be about 5855 on Rowland's scale, where D_1 = 5896·2, D_2 = 5890·2, D_3 = 5876·0.] I may record that the refractivity of the gas now under discussion is 0·132 relatively to air.

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* 'Phil. Mag.,' vol 36 (1893), p. 507.

very good agreement between his formula (1) and the observations of Holman and others upon various gases.

If the law be assumed, my observations suffice to determine the values of *c*. They are shown in the table, and they agree well with the numbers for air and oxygen calculated by Sutherland from observations of Obermayer.

Report of Magnetical Observations at Falmouth Observatory for the Year 1897. Latitude 50° 9′ 0″ N. Longitude 5° 4′ 35″ W. Height 167 feet above mean sea level.

The Declination and Horizontal Force are deduced from hourly readings of the photographic curves, and so are corrected for the diurnal variation.

The results in the following tables, Nos. I, II, III, IV, are deduced from the magnetograph curves, which have been standardised by observations of deflection and vibration. These were made with the Collimator Magnet marked 66A, and the Declinometer Magnet marked 66C, in the Unifilar Magnetometer No. 66 by Elliott Brothers, of London. The temperature correction (which is probably very small) has not been applied.

The Declination and Horizontal Force values given in Tables I to IV are prepared in accordance with the suggestions made in the Fifth Report of the Committee of the British Association on comparing and reducing magnetic observations, and the time given is Greenwich Mean Time, which is 20 minutes 18 seconds earlier than local time.

The following is a list of the days during the year 1897 which were selected by the Astronomer Royal as suitable for the determination of the magnetic diurnal variations, and which have been employed in the preparation of the magnetic tables:—

January	6, 9, 22, 23, 26	July	1, 9, 13, 18, 26
February	2, 9, 17, 18, 20	August	4, 5, 6, 24, 31
March	14, 15, 16, 18, 20	September	13, 18, 19, 26, 28
April	3, 11, 12, 15, 22	October	5, 9, 13, 20, 21
May	8, 9, 12, 16, 28	November	7, 8, 12, 23, 30
June	8, 9, 10, 12, 30	December	8, 13, 26, 27, 28

EDWARD KITTO,
Magnetic Observer.

Table I Hourly Means of Declination at the Falmouth
on Five selected quiet Days in

(18° + West)

Hours	Mid	1	2	3	4	5	6	7	8	9	10	11
						Winter						
1897												
Jan	43·5	43·7	43·[illegible]	44·2	44·0	44·[illegible]	43·9	43·8	43·3	43·3	43·[illegible]	45·4
Feb	43·0	43·1	43·2	43·5	43·4	43·5	43·[illegible]	42·7	4[illegible]·4	42·2	42·6	43·8
March	41·8	42·0	42·9	42·3	42·1	42·0	41·8	41·4	40·[illegible]	40·2	40·6	42·9
Oct	40	40·8	41	41·3	40·[illegible]	40·8	40·4	39·9	39·1	38·9	40·2	42·6
Nov	38·9	39·8	40·7	40·1	40·1	39·7	39·6	39·6	39·5	39·1	39·7	41·0
Dec	38·6	38·7	39·2	39·4	39·3	39·3	39·1	39·2	38·9	38·7	39·0	39·5
Means	41·1	41·3	41·5	41·8	41·6	41·6	41·3	41·1	40	40·2	41·0	43·5
						Summer						
April	43·1	42·9	43·2	42·7	42·5	42·7	42·1	40·8	39·0	38·8	39·9	42·8
May	43·4	43·3	43·0	42·9	42·8	42·6	40·8	39·6	38·[illegible]	39·7	41·6	44·3
June	42·4	42·4	42·0	41·7	41·6	40·2	39·1	39·0	38·5	39·6	41·6	43·6
July	42·3	42·0	41·8	41·6	40·8	39·8	38·7	38·9	38·5	38·7	40·7	42·7
Aug	40·8	41·0	40·7	40·6	40·3	39·7	38·7	38·2	38·0	38·5	40·6	43·3
Sept	39·6	39·7	39·[illegible]	39·6	39·1	39·8	39·5	39·1	38·4	38·8	40·0	42·1
Means	41·9	41·9	41·7	41·5	41·2	40	39·8	39·3	38·6	39·0	40·7	43·1

Table II Diurnal Inequality of the Falmouth

Hours	Mid	1	2	3	4	5	6	7	8	9	10	11
						Summer mean						
	−0·5	−0·5	−0·7	−0·9	−1·2	−1·7	−2·[illegible]	−3·1	−3·8	−3·4	−1·7	+0·7
						Winter mean						
	−0·9	−0·7	−0·5	−0·2	−0·4	−0·4	−0·7	−0·9	−1·5	−1·8	−1·0	+0·2
						Annual mean						
	−0·7	−0·6	−0·6	−0·6	−0·8	−1·1	−1·[illegible]	−2·0	−2·7	−2·6	−1·4	+0·5

Observatory determined from the Magnetograph Curves each Month during 189"

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid
						Winter						
46 6	47 2	46 5	45	45	45 2	44 6	44 3	43 8	43 6	43 4	43 6	43 8
45 7	47 0	46 9	46 1	45 8	44 7	44 [illegible]	44 [illegible]	43 8	43 5	43 1	42 9	42 9
45 8	48 1	48 8	47 9	46 1	44 3	43 3	42 7	42 7	42 6	42 4	41 9	42 0
44 6	45 2	44 7	44 1	4 6	42 2	41 8	41 1	41 3	40 9	40 8	40 9	40 [illegible]
42 5	42 9	42 5	41 6	41	41 0	40 2	40 1	39 8	38 9	38 3	38 3	38 5
40 3	40 7	40 5	39 7	39 6	39 2	38	38 6	39 5	38 1	38 1	38 2	38 5
44 3	45	46 0	44 2	43 3	42 8	42	41 8	41 7	41 3	41 0	41 0	41 1
						Summer						
46 5	48 3	49 0	47 7	46	45 0	43 9	43 9	43 8	43 7	43 7	43 4	43 1
47 0	47 8	48 3	46 [illegible]	45 0	44 3	43 9	43 6	43 6	43 6	43 5	43 3	43 4
45 5	46 4	46 8	46	4 2	44 1	43 9	43 3	43 0	4 9	42 9	42 1	42 0
45 4	46 3	4	46 0	44 8	43	4 3	42 2	42	42 1	42 4	42 0	42 0
46 0	47 5	47 5	45 4	44 6	42 8	41 6	41 2	41 2	41 3	41 2	41 0	41 0
44 0	45 6	45 4	44 7	43 7	42 8	42 7	42 2	41 3	41 0	40 8	40 8	40 4
46 7	47 0	47 3	46 1	45 0	43 7	43 1	42	42 5	4 4	4 4	40 1	42 0

Declination as deduced from Table I

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid
						Summer mean						
+4 3	+4 6	+4 9	+3 7	+2 6	+1 3	+0 7	+ 2	+0 1	0 0	0 0	−0 3	−0 4
						Winter mean						
+2 3	+3 2	+3 0	+2 2	+1 3	+0 8	+0 2	−0 3	0 3	−0 7	−1 0	−1 0	−0 9
						Annual mean						
+3 3	+3 9	+4 0	+3 0	+2 0	+1 1	+0 5	0 0	−0 1	−0 4	−0 5	−0 7	−0 7

Table III — Hourly Means of the Horizontal Force at Falmouth on Five selected quiet Days in

0 18000 + (C G S units)

Hours	Mid	1	2	3	4	5	6	7	8	9	10	11
						Winter						
1897												
Jan	578	578	578	580	582	583	584	584	581	576	569	567
Feb	580	579	578	578	579	580	581	581	580	576	571	569
March	584	585	585	585	585	586	588	589	584	574	566	565
Oct	607	605	603	603	606	605	606	603	598	589	583	584
Nov	598	598	599	600	602	604	605	603	603	597	591	590
Dec	587	588	586	589	592	594	596	595	595	594	588	585
Means	589	589	588	589	591	592	593	593	590	584	578	577
						Summer						
April	596	596	594	593	593	595	599	597	591	581	571	563
May	597	596	598	596	598	596	592	587	580	573	571	574
June	604	603	601	602	602	599	596	592	587	582	581	586
July	610	611	609	608	608	606	603	599	594	588	581	579
Aug	613	613	612	612	611	609	606	603	595	589	589	593
Sept	604	606	604	604	604	604	604	601	595	587	582	586
Means	604	604	603	603	602	602	600	597	590	583	579	580

Table IV — Diurnal Inequality of the Falmouth

Hours	Mid	1	2	3	4	5	6	7	8	9	10	11
						Summer mean						
	+ 00004	+ 00004	+ 00003	+ 00003	+ 00002	+ 00002	00000	− 00003	− 00010	− 00017	− 00021	− 00020
						Winter mean						
	00000	00000	− 00001	·00000	+ ·00002	+ 00003	+ 00004	+ 00004	+ 00001	− 00005	− 00011	− 00012
						Annual mean						
	+ 00003	+ ·00002	+ 00001	+ ·00002	+ 00002	+ ·00003	+ ·00002	+ 00001	− ·00006	− ·00011	− 00016	− 00016

Observatory, determined from the Magnetograph Curves
each Month during the year 1897

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid
						Winter						
568	571	579	578	579	581	583	585	584	583	581	580	579
571	576	576	578	577	579	579	580	583	581	583	583	582
566	572	576	578	582	583	585	590	591	592	590	590	588
590	596	600	602	602	603	607	608	609	609	610	610	609
590	591	594	592	597	600	602	602	603	601	601	602	601
588	591	594	593	592	592	593	596	596	595	593	593	592
579	583	587	587	588	590	592	594	594	594	593	593	592
						Summer						
566	572	575	587	595	599	599	603	604	604	604	605	605
579	588	595	598	600	605	609	608	609	604	609	605	603
591	593	593	597	600	605	608	612	613	612	612	610	607
584	591	596	605	609	614	617	618	620	618	616	614	612
595	598	600	605	612	617	618	621	621	620	618	617	616
589	595	599	602	606	604	607	611	613	611	611	611	611
584	590	593	599	604	607	610	612	613	612	611	610	609

Horizontal Force as deduced from Table III

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid
						Summer mean						
− ·00016	− ·00010	− ·00007	− ·00001	+ ·00004	+ ·00007	+ ·00010	+ ·00012	+ ·00013	+ ·00012	+ ·00011	+ ·00010	+ ·00009
						Winter mean						
− ·00010	− ·00006	− ·00002	− ·00002	− ·00001	+ ·00001	+ ·00003	+ ·0000	+ ·00005	+ ·00005	+ ·00004	+ ·00004	+ ·00003
						Annual mean						
− ·00013	− ·00008	− ·00005	− ·00002	+ ·00002	+ ·00004	+ ·00007	+ ·00009	+ ·00009	+ ·00009	+ ·00008	+ ·00007	+ ·00006

Report of Magnetical Observations at Falmouth Observatory for the Year 1898 Latitude 50° 9′ 0″ N., Longitude 5° 4′ 35″ W.; height, 167 feet above mean sea-level.

The Declination and the Horizontal and Vertical Forces are deduced from hourly readings of the photographic curves, and so are corrected for the diurnal variation.

The results in the following tables, Nos. I, II, III, IV, are deduced from the magnetograph curves which have been standardised by observations of deflection and vibration. These were made with the Collimator Magnet, marked 66A, and the Declinometer Magnet, marked 66C, in the Unifilar Magnetometer No. 66, by Elliott Brothers, of London. The temperature correction (which is probably very small) has not been applied

In Tables V and VI the Vertical Force values, also deduced from the Photographic Curves, have been standardised by observations of Dip and of Horizontal Force, and are published for the first time. The January results are based on four days' means, and the June and October results on the means of three days only. No temperature correction has been applied, and this probably has modified to some extent the apparent law of variation of the Vertical Force throughout the twenty-four hours. As is not unusual with a new instrument, some discontinuities occurred in the course of the year

In Table VII, H is the mean of the absolute values observed during the month (generally three in number), uncorrected for diurnal variations and for any disturbance. V is the mean of the products of the tangent of Dip and H.

In Table VIII the Inclination is the mean of the absolute observations, the mean time of which is 3 P.M. The Inclination was observed with the Inclinometer No. 86, by Dover, of Charlton, Kent, and needles 1 and 2, which are 3½ inches in length.

The Declination and the Horizontal and Vertical Force values given in Tables I to VI are prepared in accordance with the suggestions made in the Fifth Report of the Committee of the British Association on comparing and reducing magnetic observations, and the time given is Greenwich Mean Time, which is 20 minutes 18 seconds earlier than local time.

The following is a list of the days during the year 1898 which were selected by the Astronomer Royal as suitable for the determination of the magnetic diurnal variations, and which have been employed in the preparation of the magnetic tables :—

January	3, 4, 7, 9, 23
February	1, 3, 7, 26, 27
March	1, 3 4, 24, 31
April	1, 9, 21, 22, 29
May	7, 19, 21, 23, 25
June	5 13, 17, 20, 21
July	2, 10, 15, 16, 18
August	1 8, 10, 15, 25
September	6, 7, 12, 21, 26
October	4, 8, 12, 16, 18
November	5, 10, 14, 29, 30
December	11, 12, 17, 23, 26

EDWARD KITTO,
Magnetic Observer.

Table I.—Hourly Means of Declination at the Falmouth
on Five selected quiet Days in

(18 + West)

Hours	Mid.	1	2	3	4	5		7	8	9	10	11
						Winter						
1898												
Jan	37	37 9	38 3	38 4	38 3	38 0	37 8	37 7	37 4	37 4	37 6	38 9
Feb	37 8	38 0	38 0	38 0	38 1	38 0	37 7	37	37 5	37 1	37 3	38 7
Mar	37 9	38 0	37 9	38 0	37 7	38 4	37 9	37 7	37 1	36	36 6	38 7
Oct	34 9	35 2	35 3	35 4	35 0	35 3	35 1	34 5	33 3	33 2	34 7	37 2
Nov	35 7	35 8	36 4	5	36 5	3 4	36 9	3 1	33 1	36 0	36 8	38 1
Dec	34 9	35	3 7	36 1	35 8	3 8	3 6	35 6	35 3	35 5	36 3	36 6
Means	36 5	36 7	36 9	37 1	3 9	37 0	3 7	36 5	36 1	3 9	36 6	38 0
						Summer						
April	38 6	38 7	38 4	38 4	38	37 0	38 1	37 5	36 4	3 0	36 9	38 9
May	36 9	37 1	36 9	3 8	36 2	35 1	33 5	32 3	31	32 5	3 1	38 6
June	37 6	37 9	37 5	37 3	3 6	35 3	33 8	33 2	33	33 1	35 0	38 1
July	37 8	37 3	37 0	3 7	36 2	35 4	34 1	33 8	33 7	34 9	36 2	38 4
Aug	36 8	36 0	36 3	35 8	3 7	35 4	34 7	34 1	33 0	34 8	36 8	3 3
Sept	3 5	35 6	35 0	35 2	34 5	34 3	33 8	33 0	33 0	33 9	35 6	37 9
Means	37 2	37 3	36 9	36 7	36 2	35 6	34 7	34 2	33 8	34 1	35 0	38 5

Table II.—Diurnal Inequality of the Falmouth

Hours	Mid.	1	2	3	4	5	6	7	8	9	10	11
						Summer mean						
	0 4	-0 9	-0 7	0 9	-1 4	- 0	2 9	-3 4	3 8	3 5	-1 7	+0 9
						Winter mean						
	-0 9	-0 7	-0 5	-0 3	-0 5	-0 4	-0 7	-0 9	-1 3	-1 5	-0 8	+0 6
						Annual mean						
	-0 7	-0 5	-0 6	-0 6	-1 0	-1 2	-1 8	-2 2	-2 6	-2 5	-1 8	+0 8

Observatory determined from the Magnetograph Curves each Month during 1898

Noon	1	2	3	4	5	6		8	9	10	11	Mid
						Winter						
40 1	40 2	39 0	39 0	39	38 7	38 2	37 7	37 5	3	3 9	37 4	3 8
40 2	41 4	41 3	40 8	39 8	39 1	38 7	38 1	38 1	37 7	3	37 5	37 3
41 6	43 1	43 4	42 1	41 2	4 1	39	39 3	38 9	38 8	38 5	38 4	38 4
39 7	40 4	40	39 2	37	3 5	36 6	36 2	35 6	35 3	3 2	35 3	3 1
39 4	39 9	39 4	39 0	3 1	37	36 4	36 3	3	3 8	3	3	3 0
37 6	37 5	37 4	3 8	3	3 7	3 3	3 1	34 8	34	34 4	34 6	34 8
39 8	40 4	40 3	9	38	8 0	3	1	36 9	36 6	3 4	36 5	36 6
						Summer						
41 5	43 8	44	43 6	42 2	41	4 3	39	3	9	39 7	38 6	38 8
41 4	43 0	4 7	41 1	3 4	37 8	3		36 8	36 9	36 8	36 8	36 1
40 7	42 1	41 8	41 0	39 4	39 0	38	37 7	3 1	37 4	37 5	37 8	37 8
41 2	42	4 3	41 5	40 1	38 8	38	38	37 9	37 3	37 7	37 5	37 5
41 7	43 1	43 2	42 6	4	39 4	8	37	3 6	3 3	37 3	37 2	37 1
40 8	41 3	41 3	40	37 8	3 3	35	3 8	8	3 2	3 3	35 6	3 5
41 2	42 8	4 7	41 6	40 0	38 8	37 8	37 5	37	37 3	37 9	37 3	37 1

Declination as deduced from [illegible]

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid
						Summer means						
+3 6	+5 2	+5 1	+4 0	+2 4	+1 9	+0 2	−0 1	0 2	0 3	−0 4	−0 3	−0 5
						Winter means						
+2 4	+3 0	+2 9	+2 1	+1	+0 6	+0 1	0 3	0 5	−0 8	1 0	−0 9	−0 8
						Annual means						
+3 0	+4 1	+4 0	+3 1	+1 8	+0 9	+0 2	−0 2	−0 4	−0 6	−0 7	−0 6	−0 7

Table III.—Hourly Means of the Horizontal Force at Falmouth

Five selected quiet Days in

0·18000 + (C.G.S. units)

Hours	Mid	1	2	3	4	5	6	7	8	9	10	11
						Winter						
1898												
Jan	604	604	604	605	607	610	612	612	610	607	599	599
Feb	623	622	621	622	623	625	627	626	626	625	621	615
March	628	620	621	622	621	622	624	626	625	619	614	610
Oct	638	639	630	636	636	637	636	636	632	623	616	618
Nov	635	634	632	634	635	630	611	612	634	631	622	621
Dec	635	635	636	638	637	648	630	630	638	636	635	638
Means	626	626	626	626	627	629	630	630	628	624	618	615
						Summer						
April	622	610	620	619	618	617	617	617	613	608	600	596
May	636	634	632	630	631	630	624	615	609	602	598	599
June	639	637	635	635	635	631	630	626	618	614	610	612
July	630	620	620	628	628	628	624	616	610	605	604	611
Aug	648	640	613	641	612	639	636	632	625	619	617	620
Sept	622	624	622	621	619	618	616	618	609	603	597	596
Means	633	632	630	629	629	628	625	620	614	609	604	606

Table IV.—Diurnal Inequality of the Falmouth

Hours	Mid	1		3	4	5	6	7	8	9	10	11
						Summer mean						
	+ 00006	+ 00005	+ 00003	+ 0000	+ 0000	+ 00001	− 0000	− 00007	− 00013	− 00018	− 000 2	− 000 1
						Winter mean						
	00000	00000	00000	00000	+ 00001	+ 00003	+ 00004	+ 00004	+ 0000	− 0000	− 00008	− 00011
						Annual mean						
	+ 00003	+ 00003	+ 0000	+ 00001	+ 0000	+ 0010	+ 00001	− 0000	− 00006	− 00010	− 00016	− 00016

Observatory, determined from the Magnetograph Curves on each Month during the Year 1898

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid
						Winter						
602	606	608	606	606	608	609	612	612	611	611	611	609
615	619	621	621	621	621	621	624	624	626	625	625	626
609	613	616	617	620	621	622	626	627	627	626	627	627
619	625	633	636	636	639	641	642	643	642	641	641	640
626	632	632	633	636	639	640	643	643	642	638	637	636
635	636	637	637	639	639	640	639	640	639	639	639	637
618	622	625	625	626	628	629	631	632	631	630	630	629
						Summer						
600	606	611	614	618	623	628	628	627	626	629	626	635
601	600	617	624	629	635	639	641	644	643	641	640	636
620	625	631	634	637	640	642	644	646	644	641	641	638
617	620	624	630	632	635	637	639	638	633	638	635	634
628	631	630	634	639	645	641	653	656	655	658	654	651
604	613	615	618	620	623	623	629	630	631	630	628	627
612	617	621	626	629	634	636	639	640	640	638	637	635

Horizontal Force as deduced from Table III

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid
						Summer mean						
− 00018	− 00010	− 00006	− 00001	+ 00002	+ 00007	+ 00009	+ 00012	+ 00014	+ 00013	+ 00011	+ 00010	+ 00008
						Winter mean						
− ·00006	− 00004	− 00001	− 00001	00000	+ 00003	+ 00003	+ 00006	+ 00006	− 00005	+ 00004	+ 00004	+ 00003
						Annual mean						
− 00012	− 00007	− 00004	− 00001	+ 00001	+ 00005	+ 00006	+ 00009	+ 00010	+ 00009	+ 00008	+ ·00007	+ 00006

Table V —Hourly Means of the Vertical Force at Falmouth
Five selected quiet Days in

0 ·43000 + (C G S units)

Hours	Mid	1	2	3	4	5	6	7	8	9	10	11
						Winter						
1898												
Jan	610	611	613	613	613	612	611	609	608	607	606	603
Feb	616	616	615	616	616	615	615	614	614	614	612	606
March	614	616	618	618	619	619	620	621	622	621	615	608
Oct	561	561	565	566	566	567	566	566	565	568	556	550
Nov	555	555	556	557	557	557	556	556	555	554	552	553
Dec	531	531	531	530	530	530	530	529	529	528	528	526
Means	581	582	583	583	584	583	583	583	582	581	578	574
						Summer						
April	557	559	561	562	563	563	568	568	562	559	554	542
May	602	603	603	605	607	610	610	609	604	596	585	577
June	596	599	601	603	605	608	609	606	601	598	587	570
July	523	525	526	527	528	529	530	530	527	521	517	505
Aug	554	555	557	561	563	567	569	570	567	560	554	548
Sept	557	557	557	558	559	560	562	565	563	557	559	539
Means	565	566	568	569	571	573	574	574	571	565	559	547

Table VI —Diurnal Inequality of the Falmouth

Hours	Mid	1	2	3	4	5	6	7	8	9	10	11
						Summer mean						
	+ 00004	+ 00005	+ 00007	+ 00008	+ 00010	+ 00012	+ 00013	+ 00013	+ 00010	+ 00004	− 00002	− 00014
						Winter mean						
	·00000	+ 00001	+ 00002	+ 00002	+ 00003	+ 00002	+ 00002	+ 00002	+ 00001	00000	− ·00003	− 00007
						Annual mean						
	+ 00002	+ 00003	+ 00005	+ 00005	+ 00006	+ 00007	+ ·00008	+ 00008	+ 00006	+ 00002	− 00002	− ·00010

Observatory, determined from the Magnetograph Curves on each Month during 1898.

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid.
						Winter.						
604	607	609	611	611	612	610	609	609	607	608	609	609
605	606	609	612	615	614	613	611	611	609	609	608	606
603	606	611	617	621	623	624	624	625	625	627	629	629
550	551	553	562	566	564	564	563	564	565	567	568	569
553	555	558	559	560	558	557	556	556	555	556	555	555
526	527	532	533	532	531	530	529	528	528	527	527	526
574	575	579	582	584	584	583	582	582	582	582	583	582
						Summer						
537	538	545	553	557	560	563	562	560	559	559	559	559
575	580	586	596	602	605	606	606	601	597	598	598	598
563	570	575	582	585	590	590	587	582	578	578	580	581
498	400	504	511	514	517	517	515	515	512	513	515	518
543	542	544	530	556	559	559	558	556	556	556	558	558
530	527	531	539	544	545	545	545	545	546	543	544	544
541	543	548	555	560	563	563	562	560	558	558	559	560

Vertical Force as deduced from Table V

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid
						Summer mean						
− ·00020	− ·00018	− ·00013	− ·00008	− ·00001	+ ·00002	+ ·00002	+ ·00001	+ ·00001	− ·00006	− ·00003	− ·00002	− ·00001
						Winter mean						
− ·00007	− ·00006	− ·00002	+ ·00001	+ ·00003	+ ·00003	+ ·00002	+ ·00001	+ ·00001	+ ·00001	+ ·00001	+ ·00002	+ ·00001
						Annual mean						
− ·00013	− ·00012	− ·00007	− ·00003	+ ·00001	+ ·00002	+ ·00002	+ ·00001	·00000	− ·00001	− ·00001	·00000	·00000

Table VII —Magnetic Intensity Absolute Observations
Falmouth Observatory 1898

1898	C G S measure	
	H or Horizontal force	V or Vertical force
January	0 18603	0 43611
February	0 18600	0 43583
March	0 18585	0 43562
April	0 185[illegible]3	0 435[illegible]8
May	0 186[illegible]7	0 4[illegible]3[illegible]
June	0 18611	0 43545
July	0 18611	0 4[illegible]10
August	0 18628	0 435[illegible]4
September	0 18[illegible]02	0 43[illegible]
October	0 186[illegible]	0 43548
November	0 18624	0 43541
December	0 18630	0 43544
Means	0 18609	0 43[illegible]53

Table VIII —Magnetic Inclination Absolute Observations
Falmouth Observatory 1898

Month		Mean
January	1	66 54 5
	10	6 54 3
	28	66 52 8
		[illegible] 53
February	8	66 52 5
	21	66 55 0
	26	66 52 4
		66 53 3
March	10	66 53 0
	19	66 55 9
	29	66 52 [illegible]
		66 53 7
April	7	66 52 5
	18	66 52 4
	19	66 52 2
	20	66 52 3
	28	66 51 6
		66 52 2
May	6	66 51 8
	17	66 52 2
	27	66 50 6
		66 51 5
June	10	66 51 5
	21	66 51 3
	29	66 51 7
		66 51 5
July		66 50 [illegible]
	21	66 50 5
	20	66 50 5
		66 50 5
August	10	66 51 2
	15	66 50 6
	28	66 51 0
		66 50 9
September	4	66 51 6
	25	66 51 9
	30	66 53 9
		66 52 5
October	14	66 50 7
	21	66 51 5
	29	66 52 8
		66 51 7
November	11	66 50 8
	19	66 50 1
	29	66 50 5
		66 50 5
December	10	66 49 9
	21	66 49 4
	31	66 50 1
		66 49 8

Report of Magnetical Observations at Falmouth Observatory for the Year 1899 Latitude 50° 9 0 N Longitude ° 4 35 W height 167 feet above mean sea level

The Declination and the Horizontal Force are deduced from hourly readings of the photographic curves and so are corrected for the diurnal variation

The results in the following tables Nos I II III IV are deduced from the magnetograph curves which have been standardised by observations of deflection and vibration These were made with the Collimator Magnet marked 66A and the Declinometer Magnet marked 66C, in the Unifilar Magnetometer No 66 by Elliott Brothers of London The temperature correction (which is probably very small) has not been applied

In Table V H is the mean of the absolute values observed during the month (generally three in number) uncorrected for diurnal variations and for any disturbance V is the mean of the products of the tangent of Dip and H

In Table VI the Inclination is the mean of the absolute observations, the mean time of which is 3 P M The Inclination was observed with the Inclinometer No 86 by Dover of Charlton Kent and needles 1 and 2, which are 3½ inches in length

The Declination and the Horizontal Force values given in Tables I to IV are prepared in accordance with the suggestions made in the Fifth Report of the Committee of the British Association on comparing and reducing magnetic observations, and the time given is Greenwich Mean Time, which is 20 minutes 18 seconds earlier than local time

The following is a list of the days during the year 1899 which were selected by the Astronomer Royal as suitable for the determination of the magnetic diurnal variations, and which have been employed in the preparation of the magnetic tables —

January	1, 7, 10 13 27	February	4, 5, 7, 8, 18
March	4, 5, 26 27, 30	April	13, 15, 16, 21, 22
May	13 14 24 25 29	June	6, 7, 17, 25 26
July	15, 17, 22, 28, 29	August	12, 16, 18, 19, 23
September	5, 6, 7, 14, 20	October	2, 3, 10, 20 29
November	2, 10, 16, 20, 27	December	6, 11, 14, 15, 24

EDWARD KITTO,
Magnetic Observer

Table I Hourly Means of Declination at the Falmouth on Five selected quiet Days in

(18° + West)

Hours	Mid.	1	2	3	4	5	6	7	8	9	10	11
						Winter						
1899.	′	′	′	′	′	′	′	′	′	′	′	′
Jan ..	35·0	35·5	35·3	35·6	35·4	35·2	35·1	35·0	34·9	35·2	35·8	36·6
Feb	35·9	36·2	36·3	36·1	36·8	36·4	36·3	36·3	36·5	36·9	38·0	38·6
March	32·8	32·6	32·3	32·1	32·2	32·5	32·4	32·3	31·8	31·1	31·5	34·4
Oct .	31·0	31·1	31·1	31·0	30·0	30·8	30·6	30·0	29·2	29·0	30·0	31·9
Nov.	29·9	30·7	30·8	30·8	30·7	30·5	30·3	29·9	29·3	29·2	30·1	31·6
Dec .	30·0	30·1	30·2	30·1	30·4	30·4	30·2	30·0	30·0	30·1	30·5	31·2
Means	32·4	32·7	32·7	32·8	32·7	32·6	32·5	32·3	32·0	31·9	32·7	34·1
						Summer						
	′	′	′	′	′	′	′	′	′	′	′	′
April .	33·1	33·7	33·6	33·6	33·4	33·0	32·6	31·6	30·2	30·2	31·6	34·2
May .	31·6	31·7	31·3	31·5	31·1	30·1	28·8	27·6	27·3	28·3	30·5	33·3
June ..	32·7	32·6	32·5	32·4	32·1	30·8	29·3	28·8	28·5	28·5	30·6	33·0
July ..	32·2	31·7	31·7	31·6	31·2	30·5	29·6	29·6	28·9	29·4	31·1	33·0
August.	31·8	31·8	31·9	31·8	31·6	30·9	30·3	29·3	28·6	29·7	32·2	33·0
Sept	28·7	28·7	29·2	28·8	28·5	28·3	27·6	26·5	25·9	26·3	28·6	31·3
Means	31·7	31·7	31·7	31·6	31·3	30·6	29·7	28·9	28·2	28·7	30·8	33·3

Table II Diurnal Inequality of the Falmouth

Hours	Mid	1	2	3	4	5	6	7	8	9	10	11
						Summer mean						
	′	′	′	′	′	′	′	′	′	′	′	
	−0·7	−0·7	−0·7	−0·8	−1·1	−1·8	−2·7	−3·5	+4·2	−3·7	−1·6	+0·1
						Winter mean						
	′	′	′	′	′	′	′	′	′	′	′	′
	−0·7	−0·4	−0·4	−0·3	−0·4	−0·5	−0·6	−0·8	−1·1	−1·2	−0·4	+1·0
						Annual mean						
	′	′	′	′	′	′	′	′	′	′	′	′
	−0·7	−0·6	−0·6	−0·6	−0·8	−1·2	−1·7	−2·2	−2·7	−2·5	−1·0	+1·0

Observatory determined from the Magnetograph Curves each Month during 1899

Noon	1	2	3	4	5	6	7	8	9	10	11	M d
						Winter						
37 2	37 6	36 4	36	36 0	3 7	3	3 2	3 2	34 7	34 8	34 8	34 9
39 5	39 7		39 1	7 1	37 1	37	6	3 4	3 1	36 1	35 8	35 2
37 0	38 5	38	3 7	35 3	34 1	33 3	33 0	3 6	39 7	3 6	32 4	32 7
38 5	34 7	3	31 2	32 9	32 1	31 8	31	1	31 1	31 0	30 8	30 9
33 0	33 3	39 5	31 4	30 7	30 6	3 3	30 2	30 1	30 3	30	30 0	30 1
31 8	32 3	31 8	31 2	30 7	3 3	30 1	29 8	29 6	29 7	29 8	29 8	30 0
35 3	36	35	34 6	33 8	33 3	33	3 8	32 5	32 4	3 4	3 8	32 8
						Summer						
37 2	39 5	40 2	39	37 1	3 7	34 4	33 3	33 4	3 7	33	33 4	33 6
35 8	3	36 7	3 7	31	33 0	3 4	3		3	31 8	31 8	31 6
36 2	37	38 4	37 8	3 6	3 1	4 4	33 4	33 3	33 1	33 2	33 2	33 0
35 4	6 4	37 4	36 7	3 2	34	33 4	33 1	3	32	32	32 2	32 2
36 6	37 7	37 3	3 3	34 6	33 4	32	3 1	32	3 5	32 3	32	31 8
34 3	5 4	3 2	33 4	31 5	3	29 3	8	9 1	29 0	28 6	28 4	28 7
35 9	37 4	37 5	36 5	34 8	33 5	32 7	32 3	32 2	32 1	32 0	31 8	31 9

Declination as deduced from Table I

Noon	1	2	3	4	5	6	7	8	9	10	11	M d
						Summer mean						
+3 5	+5 0	+5 1	+4 1	+2 4	+1 1	+0 3	−0 1	−0 2	−0 3	−0 4	−0 6	−0 5
						Winter mean						
+2 2	+2 9	+2 5	+1 5	+0 7	+0 2	−0 1	0 3	−0 6	−0 7	−0 7	0 8	−0 8
						Annual mean						
+2·9	+4·0	+3 8	+2 8	+1 6	+0 7	+0 1	−0 2	−0 4	−0 5	−0 6	−0 7	−0 7

Table III —Hourly Means of the Horizontal Force at Falmouth
Five selected quiet Days in

0 1800 + (C G S units)

Hours	Mid	1	2	3	4	5	6	7	8	9	10	11
						Winter						
1890												
Jan	648	649	649	650	652	653	654	655	654	647	645	646
Feb	651	648	648	648	649	650	653	653	652	649	646	645
March	656	654	652	653	654	657	657	660	655	647	640	637
Oct	667	669	667	667	668	669	670	668	663	657	649	648
Nov	671	670	671	673	675	676	677	675	671	665	662	658
Dec	679	677	679	680	681	682	683	682	681	679	677	675
Means	662	661	661	662	663	665	666	666	663	657	658	652
						Summer						
April	661	662	661	662	660	661	660	658	654	645	638	631
May	660	668	666	665	665	663	659	652	643	636	634	636
June	667	665	665	663	662	664	660	656	651	646	648	643
July	667	666	664	665	665	663	660	657	654	651	647	645
Aug	672	670	670	669	668	666	663	658	652	647	644	646
Sept	681	681	680	680	679	678	676	670	662	653	646	649
Means	670	669	668	667	667	666	663	659	653	646	643	642

Table IV —Diurnal Inequality of the Falmouth

Hours	Mid	1	2	3	4	5	6	7	8	9	10	11
						Summer mean						
	+ 00006	+ 00006	+ 00004	+ 00003	+ 00008	+ 00002	– 00001	– 00005	– 00011	– 00018	– 00022	– ·00022
						Winter mean						
	00000	– 00001	– 00001	00000	+ 00001	+ 00008	+ 00004	+ 00004	+ 00001	– 00005	– 00009	– ·00016
						Annual mean						
	+ 00008	+ 00002	+ 00002	+ 00002	+ 00002	+ 00008	+ 0000	– ·00001	– 00008	– 00012	– 00016	– 00016

Observatory, determined from the Magnetograph Curves on each Month during the year 1899

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid
						Winter						
649	653	652	648	645	647	650	650	650	649	649	648	647
646	649	652	651	649	649	651	654	656	656	656	655	657
641	649	654	655	655	656	656	659	658	658	656	657	658
648	653	657	662	661	668	669	671	671	671	672	669	669
661	665	669	672	675	676	676	676	675	674	673	672	672
677	678	678	678	679	681	683	682	682	682	685	681	682
654	658	660	661	661	663	664	665	665	665	665	664	664
						Summer						
632	639	650	658	660	664	668	663	666	666	664	664	665
648	657	663	666	665	666	671	674	676	674	672	670	670
646	649	654	662	666	669	673	677	677	676	673	671	670
650	653	661	665	665	668	669	674	676	674	674	671	670
649	657	664	666	669	670	673	680	680	679	677	677	676
660	670	676	681	682	684	684	687	685	683	683	684	682
648	654	661	666	668	670	673	676	677	675	674	678	672

Horizontal Force as deduced from Table III

Noon	1	2	3	4	5	6	7	8	9	10	11	Mid
						Summer mean.						
− 00016	− 00010	− 00003	+ 00002	+ 00004	+ 00006	+ 00009	+ 00012	+ 00 13	+ 00011	+ 00010	+ 00009	+ 00008
						Winter mean						
− 00008	− 00004	− 00002	− 00001	− 00001	+ 00001	+ 00002	+ 00003	+ 00003	+ 00003	+ 00003	+ 00003	+ 00003
						Annual mean						
− 00012	− 00007	− 00003	+ 00001	+ 00001	+ 00004	+ 00006	+ 00006	+ 00008	+ 00007	+ 00007	+ 00006	+ ·00005

Table V.—Magnetic Intensity. Absolute Observations.
Falmouth Observatory, 1899.

1899	C G S measure	
	H or Horizontal force	V or Vertical force
January	0 18636	0 43558
February	0 18645	0 43548
March	0 18631	0 43543
April	0 18642	0 43[illegible]
May	0 18640	0 43[illegible]
June	0 18639	0 43474
July	0 18655	0 43515
August	0 18662	0 43525
September	0 18654	0 43572
October	0 18655	0 43498
November	0 18660	0 43518
December	0 18656	0 43545
Means	0 18647	0 43531

Table VI.—Magnetic Inclination. Absolute Observations.
Falmouth Observatory, 1899.

Month		Mean	Month		Mean
January	15	66° 49 5	July	8	66° 48 4
	23	66 51 0		21	66 46 9
	31	66 50 0		29	66 47 8
		66 50 2			66 47 7
February	10	66 48 6	August	10	66 47 3
	18	66 49 3		22	66 49 1
	27	66 50 1		29	66 46 1
		66 49 3			66 47 5
March	10	66 50 7	September	9	66 48 8
	24	66 51 3		23	66 48 6
	30	66 48 9		30	66 50 8
		66 50 1			66 49 4
April	7	66 48 9	October	11	66 47 0
	14	66 47 9		21	66 46 9
		66 48 4		31	66 47 6
					66 47 2
May	2	66 49 8	November	11	66 48 9
	8	66 49 1		20	66 48 1
	17	66 50 3		29	66 48·0
	27	66 48 3			66 48 3
		66 49 4			
June	8	66 48 4	December	11	66 48 5
	19	66 47 4		19	66 48 4
	29	66 48 2		29	66 48 6
		66 48·0			66 48·5

Data for the Problem of Evolution in Man. V. On the Correlation between Duration of Life and the Number of Offspring.

By Miss M. BEETON, G. U. YULE, and KARL PEARSON, F.R.S., University College, London.

Received April 19,—Read June 14, 1900.

1. According to the Darwinian theory of evolution the members of a community less fitted to their environment are removed by death. But this process of natural selection would not permanently modify a race if the members thus removed were able before death to propagate their species in average numbers. It then becomes an important question to ascertain how far duration of life is related to fertility. In the case of many insects death can interfere only with their single chance of offspring; they live or not for their one breeding season only.* A similar statement holds good with regard to annual and biennial plants. In such cases there might still be a correlation between duration of life and fertility, but it would be of the indirect character which we actually find in the case of men and women living beyond sixty years of age: a long life means better physique, and better physique increased fertility. On the other hand, there is a direct correlation of fertility and duration of life in the case of those animals which generally survive a number of breeding seasons, and it is this correlation which we had at first in view when investigating the influence of duration of life on fertility in man. The discovery of the indirect factor in the correlation referred to above was therefore a point of much interest. For it seems to show that the physique fittest to survive is really the physique which is in itself (and independently of the duration of life) most fecund.

In continuing our study of the inheritance of longevity,† it occurred to us that it would be possible at the same time as extracting data for duration of life to extract data bearing on the size of the family. Accordingly Miss M. Beeton in working upon family histories made records of this additional character. Meanwhile Mr. G. U. Yule, who had been independently at work on this very point, drew my attention again to the matter in connection with a passage in the 'Grammar of Science.'‡ We agreed to unite our material, and the result is the following joint paper.§

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* Of course longer life may denote greater chance of male or female meeting female or male, but in this case we have not a *graduated* fertility; the individual is or is not once fertile.

† 'Roy. Soc. Proc.,' vol. 65, p. 290.

‡ Second edition, p. 445.

§ We have also to very heartily thank Mr. L. N. Filon, M.A., and Mr. K. Tressler for aid in the calculations and in the preparation of diagrams.

2. The data dealt with in this paper consist of four series, the first three collected and reduced by Miss M. Beeton, and the fourth series by Mr. G. U. Yule. The sources from which they were extracted are the following:—

Mothers. Length of Life and Size of Family.

Series I.—Taken from the 'Whitney Family, of Connecticut,' a well-known history of an American Quaker family. In order to complete a thousand and more entries some very few additions were made from the 'Backhouse Family,' the history of an English north-country Quaker family. This series may be taken to substantially represent American women more or less closely connected with one strain of blood, either by inheritance or by marriage.

As soon as these results were tabled it was noticed that the average age at death of mothers was immensely below the average age at death of Englishwomen. Further, the maximum frequency of deaths which occurs at 35 to 40 was actually greater than the maximum which occurs between 70 to 75! Either then American women of this class die very early, or the women of the Whitney family suffer under some hereditary taint, *e.g.*, phthisis.

Series II.—Taken from purely English Quaker records. The data for this series were drawn from a great variety of histories and records most kindly placed at our disposal by Mr. Isaac Sharp, Secretary of the Society of Friends, and by the Secretary of the well known insurance office, the Friends' Provident Association, both of whom we desire to cordially thank for their aid. The object here was to avoid the selection which may unconsciously be made when the data are drawn from the records of a single family.* In these two series, as in the third series, we selected the records of the Society of Friends because—

(*a*.) They appear to be the most trustworthy and complete of the family histories available.

(*b*.) The ages at death of the women are given; these are rarely recorded in other genealogical works.

(*c*.) The artificial limitation of fertility seems to be less probable in a strongly religious community like the Friends than in other classes of the population.

In this series the mean age at death, the modal age, and other constants are quite fairly in accord with what we know of the population at large.

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* Of course a "family" history like that of the Whitney family, professing to deal with all the descendants of a single pair, really contains an immense addition through marriage of other strains.

Fathers. Length of Life and Size of Family.

Series III.—The great bulk of the data was extracted from the American Whitney Family Here the features noted for the women were again observed in the men, but to a much less marked degree. There was a rather high maximum frequency of death at 45,* but not so high as the maximum at 75, and the average age at death was somewhat lower than we find for the general English population. On the whole the series is a very good one

Series IV—Extracted from Burke's 'Landed Gentry.' It has been stated elsewhere† that this is a good class for such data. It possesses a higher average fertility than the Peerage, and is a class in which there is probably comparatively little artificial restriction. Unfortunately it offers no material for the age at death of women.

3. The following are the chief results obtained from the reduction of these series -

I —Table of General Results

Series	Parent	Mean age at death	Mean size of family.	S. D.		Correlation fertility and duration of life	Regression		
				Age at death	Size of family		Whole table	Life 50 years and under	Life 50 years and over
I	Mother	53·292	5·269	4·091	3·409	0·5001	0·4174	0·8085	0·2287
II	Mother	61·183	5·811	4·769	3·170	0·3174	0·2191	0·7089	0·0941
III	Father	58·088	5·400	4·213	3·153	0·4928	0·5382	0·8414	0·2186
IV	Father	63·577	5·316	5·037	3·387	0·2010	0·2240	0·5040	0·0720

In this table the unit for the standard deviation of the age at death is 5 years, the unit of the grouping in the accompanying tables. Thus age at death of mothers 35 gives the frequency of all the group of mothers dying between 32·5 and 37·5. Of course the age at death of certain parents would lie exactly on the boundary of a group, but such exact information is very rarely forthcoming, and when it is in a few cases forthcoming, *i.e.*, the day of both birth and death is given, it is very improbable that the age of death exactly bisects the year. Thus no fractionising was found necessary in the first three tables. In the 'Landed Gentry,' owing to the nature of the record, Mr. Yule found a small amount of fractionising necessary, and this appears in the table for Series IV. In the regression coefficients above tabulated 5 years is again the unit, and the coefficient of regression is the constant by which the deviation in the age at death from the mean age at death, measured in 5-year units, must be multiplied in order to obtain the probable deviation of the family from the mean family.

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* The existence of a modal value about 45 has been already noted in the resolution of the mortality curve; it is the mode of the middle age mortality component. See 'Phil. Trans.,' A, vol. 186, p. 403, and Plate 16.

† 'Phil. Trans.,' A, vol. 192, p. 257.

II. Table of Regression Formulæ or Curves.

y = Size of Family, x = Duration of Life.

Series I. American Mothers.

(*a*) For all lives. Straight line:

$$y = 0\cdot8211 + 0\cdot083,472x$$

(*b*) For lives of 50 years and under. Straight line:

$$y = 1\cdot9881 + 0\cdot163,233x$$

(*c*) For lives of 50 years and over. Straight line:

$$y = 3\cdot5531 + 0\cdot044,748x$$

Origin of x at birth and unit of x one year.

(*d*) For all lives. Cubical parabola. Origin of x at 55 years and unit = 5 years:

$$y = 6\cdot0208 + 0\cdot328,471x - 0\cdot035,056x^2 + 0\cdot003,000x^3$$

Series II. English Mothers.

(*a*) For all lives. Straight line:

$$y = 3\cdot1781 + 0\cdot043,819x$$

(*b*) For lives of 50 years and under. Straight line:

$$y = -0\cdot6222 + 0\cdot140,581x$$

(*c*) For lives of 50 years and over. Straight line:

$$y = 4\cdot9341 + 0\cdot018,810x$$

Origin of x at birth and unit of x one year.

(*d*) For all lives. Cubical parabola. Origin of x at 57·5 years and unit = 5 years:

$$y = 6\cdot4092 + 0\cdot079,120x - 0\cdot052,719x^2 + 0\cdot005,717x^3.$$

Series III. American Fathers.

(*a*) For all lives. Straight line:

$$y = -0\cdot6819 + 0\cdot105,644x$$

(*b*) For lives of 50 years and under. Straight line:

$$y = -2\cdot6766 + 0\cdot168,277x$$

(*c*) For lives of 50 years and over. Straight line:

$$y = 3\cdot3976 + 0\cdot043,726x$$

Origin of x at birth and unit of x one year.

(*d*) For all lives. Cubical parabola. Origin of x at 55 years and unit = 5 years:

$$y = 5\cdot8187 + 0\cdot363,122x - 0\cdot047,438x^2 + 0\cdot003,035x^3.$$

Series IV. *English Fathers.*

(*a*) For all lives. Straight line.

$y = 2\cdot4877 + 0\cdot044{,}800x$

(*b*) For lives of 50 years and under. Straight line.

$y = 1\cdot0061 + 0\cdot118{,}800x$

(*c*) For lives of 50 years and over. Straight line.

$y = 4\cdot6717 + 0\cdot014{,}400x$

Origin of x at birth and unit of x one year.

(*d*) For all lives. Cubical parabola. Origin of x at 60 years and unit = 5 years.

$$y = 5\cdot5075 + 0\cdot153405x - 0\cdot011940x^2 + 0\cdot003636x^3$$

The constants of the straight lines for all these series have been found at once by fitting the best straight line to the observations, *i.e.* by using the regression formula

$$y - \text{mean } y = \text{coefficient of regression } \left(\text{or } r\frac{\sigma_y}{\sigma_x}\right) \times (x - \text{mean } x).^*$$

The cubical parabolas have been fitted by the method of moments.†

The whole of this system of formulae has been plotted and is exhibited graphically in the accompanying diagrams (pp. 176–179). These diagrams suffice to give the entire graphical solution of this problem to an exactness sufficient for most practical purposes. A careful examination of these diagrams will enable the reader to follow our general conclusions even more clearly than inspection of the algebraic formulae.

I. *General Conclusions.* (i.) The regression straight line for all lives, *a*, does not give a satisfactory picture of the relation between age at death of a parent and the average number of offspring. We see at once that it is too steep at the beginning and not steep enough at the end of life. Accordingly, starting from 50 years as the sensible limit to woman's child-bearing period, the mothers were broken up into two groups, and the regression lines calculated separately for lives of 50 years and under and for lives of 50 years and over. In this way quite a reasonable fit was obtained to the observations. For convenience, the age of 50 was also taken as a dividing age for fathers. In all four cases the regression line *c* for parents living beyond 50 years shows a quite sensible deviation from the perpendicular, or *fertility is correlated with longevity even after the fecund period is passed.*

If we take American mothers there is no doubt of this increasing fertility even up to 90 years of age. With English mothers it is less marked, but appears to be quite true up to 75 years. Beyond 75 there appears to be a slight decrease. Turning to the two series for fathers we see that we might possibly have better taken 60 than 50 as a dividing age, for the general trend of the observations is much the same up to 60 years. After this there is still a sensible trend in the American results, so that aged fathers are again the most fertile. With the English fathers this relation is, as in the case of English mothers, far less marked, although it is sensible if we take fathers above 50 years.

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* See Yule, 'Roy. Soc. Proc.,' vol. 60, p. 477.

† I have shown in a memoir not yet published (*a*) how to fit all types of curves, but particularly parabolas of any order by the method of moments, and (*b*) that such method gives results practically of the same order of exactness as those given by the method of least squares.—K. P.

Thus I think we might sum up: That the peculiar physique in both men and women which leads to longevity is also associated with greater fecundity. Of two women who both live beyond 50 years, the longer lived is likely to have had before 50 the larger family. The association is, however, much greater for American than English parents, although the American parents dealt with are, in the great majority of cases, of Anglo-Saxon race. Climate, mode of life, generally selection and environment, seem to be differentiating in this respect the English and the Anglo-American. The English Friends, we should suppose, would be a class very comparable with the American Friends, yet their average life is longer, their fertility greater, and there is less association between longevity and fecundity. In both cases our algebraical formulæ show that American men and women are more alike, and English men and women are more alike than the women to the women or the men to the men of the two races. This is the more remarkable, as the English Friends as a class are by no means identical with the Landed Gentry.

(ii) In order to represent the *continuous* change in the regression, which cannot be done by two straight lines, which only enable us to distinguish the fecund and non-fecund periods of life, the statistics were fitted with cubical parabolas. The regression line at any age in life may then be looked upon as the tangent to the cubical parabola at that age. An inspection of Diagrams 3, 4, 7, 8 shows what an excellent expression such parabolas are for these statistics.

For American mothers and fathers we see dy/dx consistently positive throughout life, and we have a most excellent graphical demonstration of the physical characters which tend to longevity being also associated with fecundity. In the English fathers the same feature appears in a much less marked degree; there is a point of inflexion in the curve, although dy/dx remains positive. Up to about 75, however, the number of offspring continues to increase with duration of life, and when we break off at 95, the curve has got a renewed outward trend. With English mothers, however, the curve has a small but sensible trend inwards in old age. For fifteen years after the climacteric increased life connotes larger family, *i.e.*, shows fecundity associated with the physique peculiar to longevity, but beyond 65, as judged by the parabola, longevity is slightly unfavourable to fecundity.*

The following are the values of the regression coefficients obtained by differentiating the cubical parabola and referring to birth as origin and a year as unit:—

Table III.—Regression Coefficients showing their Change with Duration of Life.

Series	Old method, line *aa*	Cubical parabola
I	0·0835	$0\cdot437,741 - 0\cdot010,7240z + 0\cdot000,0720z^2$
II	0·0438	$0\cdot711,949 - 0\cdot019,9955z + 0\cdot000,1872z^2$
III	0·1056	$0\cdot501,698 - 0\cdot011,8074z + 0\cdot000,0728z^2$
IV	0·0448	$0\cdot546,143 - 0\cdot018,8269z + 0\cdot000,0873z^2$

By simply substituting the number of years of life z, we can find the value of the regression at any age.

5 *Illustrations of these Results.* (i.) What is the probable family of an English mother dying at 40?

> (*a*) gives 4·93, (*b*) 5·00, and (*d*) 5·24, all of which might equally well have been read off on the diagrams. The actually observed number is considerably in excess of all these, *i.e.*, 6·23. In fact, if an English mother lives to 40 years, she will, on the average, have very nearly completed her family. For an American woman (*a*) gives 4·16, (*b*) 4·54, and (*d*) 4·64. But if she lives another ten or twenty years she will probably have a family of 5 or even 6.

(ii.) Compare the strength of the relationship between duration of life and size of family for American fathers dying at 40 and 70 respectively.

We find the slope of the cubical parabola at the points corresponding to 40 and 70 years to be 0·1459 and 0·0249 respectively. The mean regression for the whole of life is 0·1056; for the first fifty years 0·1683, and for the last fifty 0·0437 (see Table I, and reduce to year as unit). It thus appears that the influence of mere number of years as compared with the physique which tends to longevity has an effect on fertility of about 5 or 6 to 1.

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* It has been suggested that this is due to the nature of the record, there being a tendency to enter only the children who survive their parents. Thus the longer the latter live the fewer would be the offspring entered. In other words, we should be under-estimating the correlation between fertility and longevity. But the Quaker birth-records include all children, and their system is uniform. There does not appear any reason on this ground for English and American returns differing so sensibly.

(iii.) Weismann has suggested that it may be an advantage to a species that its duration of life should be shortened. This is not, *à priori*, confirmed for the case of man in the American series: the longer the parents live the greater the number of their offspring. But if we can lay any stress on the bend-in for the English mothers, and on the similar but less marked tendency for the English fathers, we might argue that reproductive selection was possibly in England working against extreme longevity, although favouring parents living till 65 or 70. Indeed those who rush rapidly to brilliant but not over-stable conclusions might emphasise Weismann's views by showing how in an old community, with much greater pressure on the material resources, there is a tendency to reduce the fertility of the long-lived parents, while in a new community, with plenty of food and occupation for all, the longest-lived parents are the most fertile! However, all that we can safely say is that there is a marked difference between English and American parents, and that this distinguishing characteristic is almost equally visible if we take opposite sexes of such diverse classes as English Friends and English country gentlemen. We would leave to further investigations its true interpretation.

6. Admitting a substantial correlation between length of life and fertility, it is of great interest to investigate what effect, other things being equal,* reproductive selection would have in modifying the duration of life.

The following table gives the mean length of life of parents taken singly and of parents weighted with their offspring:—

Table IV.—Mean Duration of Life of Parents in Years

Series	Unweighted parents	Weighted parents.	Progression
I	53·292	59·920	6·628
II	61·183	63·839	2·656
III	58·086	63·082	4·996
IV	63·577	65·510	1·930

Now these are substantial differences even in the case of the English parents (II and IV), but they are very large differences in the case of the American parents (I and III). If we suppose no assortative mating on the basis of characters tending towards longevity, then it is easy to obtain a rough approximation to the effect of reproductive selection in modifying the duration of life. It has been shown* that if there be no assortative mating the average deviation, h_1, of an array of offspring from the mean of the general population of offspring due to parents deviating h_2 and h_3 from the means of the general populations of parents is given by

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* Omitting, for example, the effect of natural selection as evidenced possibly in a greater death-rate in large families, &c.

$$h_1 = r_{12}\frac{\sigma_1}{\sigma_2}h_1 + r_{13}\frac{\sigma_1}{\sigma_3}h_2,$$

where r_{12} and r_{13} are coefficients of parental inheritance, and σ_1, σ_2, σ_3, the standard deviations in offspring and parents for the character in question. When that character is longevity our data are not yet complete, but two of us have shown that the value of $r_{12}\sigma_1/\sigma_2$ for father and son, [illegible], the regression coefficient for inheritance of duration of life, is about 0·1682,† if the sons die having lived at least 21 years. We have not yet completed our data for the inheritance of the duration of life in the case (i) of minors, or (ii) in the case of the female line, although we have nearly reached the requisite amount of material. Hence the following statements must be taken as tentative and suggestive only. We will assume 0·1682 to be the regression coefficient for both sexes, and *for all ages of the offspring, minors or adults*. In this case if m_1 be the mean of the unweighted and m_2 of the weighted fathers, m_1' of the unweighted and m_2' of the weighted mothers, we should expect an increased duration of life in the offspring due to reproductive selection of

$$\begin{aligned} h_1 &= 0{\cdot}1682\,(m_2 - m_1) + 0{\cdot}1682\,(m_2' - m_1') \\ &= \left.\begin{matrix} 0{\cdot}1682\,(6{\cdot}628 + 4{\cdot}996) \\ 1{\cdot}9551 \end{matrix}\right\}\text{for the Americans} \\ &= \left.\begin{matrix} 0{\cdot}1682\,(2{\cdot}656 + 1{\cdot}900) \\ 0{\cdot}7714 \end{matrix}\right\}\text{for the English} \end{aligned}$$

Thus the increased duration of life would be about 2 years per generation from the American data, and about 9 to 9·5 months per generation from the English data.

The result for the American series shows us how an especially low expectation of life, due possibly in this case to some family character,‡ will be rapidly raised by reproductive selection, if there be no opposing factor of evolution The English results on the other hand show us a small but sensible tendency in reproductive selection to prolong the duration of life Allowing three generations to a century we might expect the duration of life to be raised about 2 years in a century by this factor of evolution

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* 'Phil. Trans.,' A, vol. 187, p. 288.

† 'Roy. Soc. Proc.,' vol. 65, p. 297. The Landed Gentry would appear to be closer than the Peerage to our present material.

‡ It is by no means certain that this is the true view of the case. We have seen that the American women have their maximum mortality in early middle-life, and only a secondary maximum at 70. The maximum mortality of the table prepared by J. P., F.R.S., for the years 1728–57 ('A Collection of the Yearly Bills of Mortality from 1657 to 1758 inclusive,' London, 1759) occurs about 41 years, and there is no evidence of a maximum at 70 at all. Thus the American data appear to resemble London data of two centuries back.

In making this statement we are supposing that parents are not a short lived selection out of the general adult population There seems no reason why they should be and we have some statistics to show they are not Thus for the Peerage and Landed Gentry we have shown that for fathers and sons living 20 or 25 years and upwards the age at death of the father is substantially greater than that of the son * Further from data for the Society of Friends Miss Beeton has found the average age at death of women in general to be 59 831 and the average age of mothers at death to be 59 793 sensibly the same In the table for 1871 to 1880 given by the Registrar General the expectation of life of women in general at 20 years of age is given as 41 66 years or the average duration of life is 61 66 years This is only very slightly greater than our average† for English mothers above, i e 61 183 and substantially less than our average for mothers weighted with their offspring i e, 63 082 years Again the general population of males of 20 had (1871 80 returns) an average life of 64 48 years which is not comparable with our Landed Gentry's sons surviving 20 with an average life of 60 915 years but with that of their fathers i e, 65 96 years We do not think therefore that parentage in particular maternity corresponds to any shortening in the expectation of life Thus reproductive selection appears to indicate a real increase in the expectation of life Such an increased expectation of life is usually considered to have come into existence during our century owing to better sanitary conditions, greater care of the sick and invalided &c &c Its exact estimation is a matter of some difficulty We find F G P Neison ‡ working on the Registrar General's returns before 1841 gives (Table D, p 8) expectations of life from 10 years onwards For males of 20 and 25 his mean durations of life are 60 69 and 62 35, for females of 20 and 25 61 60 and 63 36 respectively These are not substantially less than the Registrar General's returns for 1881 to 1890 which gives males 60 27 and 61 28, females 62 42 and 63 50 respectively In fact, the males show reduction If we stick to the Registrar General's returns as given for three different periods, and presumably more comparable with each other than with Neison's work, we have the following results —

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* 'Roy Soc Proc vol 65 p 297

† The average age at death of mothers must in our case closely give the expectation of life of women of 20, for there are few marriages below 20 and we have in our tables included all cases of sterile unions

‡ Contributions to Vital Statistics,' London 1846

Expectation of Life.

Age.	1838-54.		1871-80		1881-90	
	♂	♀	♂	♀	♂	♀.
0	39·91	41·85	41·35	44·62	43·66	47·18
20	39·48	40·29	39·40	41·66	40·27	42·42
25	36·12	37·04	35·68	37·98	36·28	38·50

Here there is an increased expectation at birth for males, but very small increases between the first and last periods at 20 and 25. For females there is an immense increase at birth and sensible increase in the other cases. Possibly a good deal of this may be due to more exact returns for the ages of women being now obtainable.

If we take the earliest Table of the Probabilities of Life, that deduced by J. P., F.R.S., for the bills of mortality in London for 1728 to 1757, and printed in the work cited on column 5, we find the number of deaths of 1000 persons born given for each year of life, male and female being combined. According to the Registrar-General's returns for 1881-90 of 1000 persons born, 728 survive to 20 and 709 to 25, but from J. P.'s table only 485 survive to 20 and 448 to 25. This tremendous mortality of infancy and youth was probably largely a *selective* death-rate. We find accordingly the expectation of life at birth to be only 25·59 years; at 20, however, to be 49·56 years; and at 25 to be 51·30 years.* These results are for London, not for England in general, but making all possible allowances for the difference between city and country, they suggest a most stringent selection. An increased expectation of life at birth of anything like 25 years in less than two centuries could not be achieved even at the American rate of two years per generation. Nor is it possible that the whole of the increase in the Registrar-General's returns for expectation of life at birth for the periods 1838-54 and 1881-90—an increase of somewhere about four to five years—could be due to reproductive selection, unless we suppose the correlation between age at death of minors and of their parents to be considerably greater than 0·1682. On the other hand, if we confined our attention to adults of 20 to 25 of both sexes, we have, roughly, an increase of about a year in the expectation of life, and this result with nine months per generation could easily have been reached within the two-generation period in question. Generally, we may conclude that the data are not very suitable for real purposes of comparison, but that there is nothing in them opposed to the suggestion that a sensible part of the increased duration of life of this century may be due to the inheritance of longevity and the correlation of longevity with fertility Further determination of the inheritance of duration of life in the case of minors may help to throw additional light on the matter

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* We do not know how J. P.'s table was deduced, but we got the above results by averaging the years lived by those surviving at any age out of the 1000 born.

7. The following method of illustrating the influence of longevity on fertility may serve to impress the matter on the reader —

In Series I the longer-lived moiety of the mothers produce 64·0 per cent. of the children, and the shorter-lived moiety 36·0 per cent

In Series III the longer lived moiety of the fathers produce 61·1 per cent. of the children, and the shorter lived moiety 38·9 per cent

In Series II the longer lived moiety of the mothers produce 55·2 per cent of the children, and the shorter-lived moiety 44·8 per cent.

In Series IV the longer-lived moiety of the fathers produce 53·5 per cent of the children, and the shorter-lived moiety 46·5 per cent

Thus, while the results are all very sensible, those for the American parents are markedly so. In both American and English statistics the influence of longevity on the fertility of the mother is greater than its influence on the father

8 *Concluding Remarks*.—A somewhat widespread view of evolution stops at the survival of the fitter without discussing the mode whereby the less fit leave no, or fewer, offspring than the fit Of course, if the unfit are exterminated before adult life, there is no chance of their reproducing themselves. It has been shown in the second paper of this series that a selective death-rate does exist for adults, so that the whole work of selection does not take place before the reproductive stage is reached. But Miss Beeton's data for the correlation of duration of life in the case of brethren dying as minors seem to show that the selective death-rate for children is rather less than greater than its value for adults.* Hence, for the reduction or extermination of stock unsuited to its environment, we should have to look largely to selection in the adult state. In the present paper we have made what we believe to be the first quantitative determination of how a selective mortality reduces the numbers of the offspring of the less fit relatively to the fitter. In the case of life under wild conditions, the correlation between fertility and power of surviving would probably be far greater. But for such life it is almost impossible to get statistics of this nature; we are thrown

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* The matter is still under investigation, so that this conclusion is stated subject to modification. Of course, the selective death-rate among children may largely remove those not weak from *inherited* constitution, but by physical or physiological accident. These our method of investigation would throw into the non-selective death-rate.

back upon measuring the effect in man, and thus obtaining what may well be considered as a minimum value of the influence under discussion

In the course of our investigations we have seen that the relationship between fertility and duration of life does not cease with the fecund period. We thus reach the important result that characters which build up a constitution fittest to survive are also characters which encourage its fertility. This result is of great value from the standpoint of the differentiation of type, where it is absolutely necessary that the fittest to survive should also be the most fertile * On the other hand, we note that duration of life is a character capable of modification by reproductive selection, and we suggest that a considerable part of the increased expectation of life observed in recent years may be due to this cause In the case of the American statistics, we see at once how it can replace a remarkably short-lived stock by a longer lived stock, the bulk of the offspring coming from the longer lived members.

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* 'The Grammar of Science,' second edition, pp. 448 –9.

Table V.—Mothers and Offspring. Series I.

Age of Mother at Death

Number of offspring	20	25	30	35	40	45	50	55	60	65	70	75	80	85	90	95	100	Totals
0	3	3	1	1	3	5	2	0	0	3	3							24
1	19	47	25	9	6	5	[illegible]	2	4	4	3	1	1					130
2	5	20	26	13	8	10	8	6	5	8		1	0	2	2	1		122
3	1	7	21	21	15	12	9	5	5	8		6	13	5	1	0		134
4	1	8	15	15	15	9	8	5	6	6	6	3	4	8	2	0		111
5		1	6	14	10	10	5	4	11	5	19	10	9	7	1	1		106
6		1	4	12	7	8	3	6	6	[illegible]	5	6	11			0		85
7			1	18	5	8		8	10	5	6	5	9	2	3	2	2	91
8				3	8	6	6	5	6	5	9	12	5	10	2	1	1	81
9				2		6	3	[illegible]	7	10	10	9	6	2	6	1	0	7
10				1	4	4	4	3	3	3	6	13	8	7	2	0	0	58
11					0	4	0	0	1	2	2	6	5	1	2	0	0	24
12					0	0	[illegible]	1	3	2	5	2	3	4	1	1	0	14
13					1	0	1	6	1	9	1	3	4	1	0	0	1	15
14					0	0	0	0	0	2	3	0	0	1	0			6
15					0	0	1	0	1	0	0	0	0	0	0			2
16					1	0	0	1	0	0	0	0	0	0	0			2
17							0	0	0	1	0	0	0	0	1			2
18							1	0	0	0	0	0	0	0	1			2
Mothers totals	29	87	99	109	90	87	64	54	69	73	83	77	78	59	26	[illegible]	4	1095
Offspring totals	36	151	261	478	450	437	370	331	438	447	547	590	547	398	212	50	85	5776

Table VI.—Mothers and Offspring. Series II

Age of mother at birth

Number of offspring	20	25	30	35	40	45	50	55	60	65	70	75	80	85	90	95	100	105	Totals
0	0	0	0	0	0	0	2	0	2	1	2	0	2	2	0	1			12
1	5	19	17	5	5	4	4	2	7	6	6	7	[illegible]	5	4	0			103
2		10	12	5	1	7	7	4	9	8	11	6	5	3		3			95
3		4	7	7	4	1	5	3	10	12	8	7	9	4	3	0			84
4		1	7	12	7	4	9	7	7	8	9	13	9	8	4	1			106
5		0	3	11	18	10	6	15	5	15	12	13	12	11	3	0			134
6		1	5	8	5	7	7	4	4	9	8	13	9	9	6	0	0	1	96
7			2	8	5	5	2	5	6	9	13	12	11	10	5	1	1		95
8			1	5	2	14	0	1	10	8	6	8	8	10	4	1			78
9			3	0	8	3	2	9	3	7	10	13	4	9	5	0			76
10				2	3	3	2	5	3	8	10	10	6	4	1	0			57
11					4	1	2	2	4	5	5	4	3	3	1	0			34
12					1	1	1	4	2	4	8	5	3	2	0	0			26
13					1	2	0	1	1	2	8	1	3	1	1	0			16
14					0	0	1	0	1	3	4	1	1	0	0	0			11
15					0	0	0	0	1	0	1	0	1	0	1	1			5
16					1	0	0	0	0	0	0	0	1	0	1				3
17								0	0	0	0	2							2
18								1	0	0	0	0							1
19												1							1
20												1							1
Mothers, totals	5	35	57	63	65	62	50	63	74	105	111	117	94	81	44	8	1	1	1036
Offspring totals	0	61	184	303	405	382	241	421	419	660	725	804	584	505	267	40	7	6	6020

Table VII —Fathers and Offspring Series III

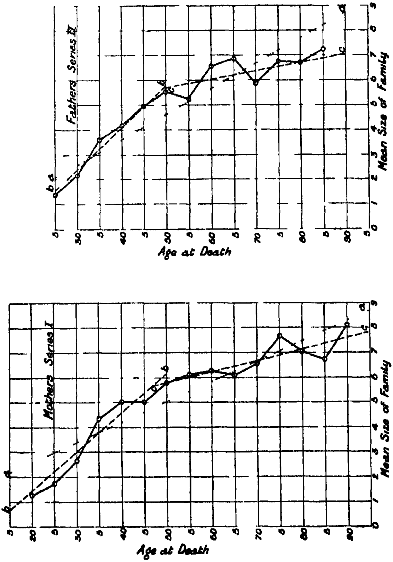
Age of father at death

Number of offspring	20	5	30	35	40	45	50	55	60	65	70		80	85	90	95	100	Totals
0	0	7	4	1	0	4	3	4	2	2	4	4	3	0	0	0	0	38
1	1	24	15	5	11	13	4	8	3	5	4	0	1	1		0	0	9
2	1	9	24	16	10	11		8	10	6	6	8	6	2	0	0	0	1[illegible]4
3		3	12	14	12	7	7	8		6	10	[illegible]		5	0	2	1	11[illegible]
4		1	4	13	14	8	15	11	4	8	9	11	4	6	1	0	1	110
5		1	4	12	9	11	8	12	11	11	18	10	10	2	1	1	0	121
6				5	10	6	6		10	6	8	10	10	8	1	0	1	[illegible]8
7				2	6	9	3	7	10	11	8	8	9	9	1	1	0	84
8				1	2	6	7	5	10	8	9	8	12	11		2	0	83
9				1	2	6	4	5	6	10	13	13	5	5	1	1	0	72
10					1	5	6	7		8	[illegible]	7	6	4	1	2	1	61
11					1	4		9	8	5	0	7	4		1	1		40
1						1	3	2		4	1	4	5	2	0	1	1	26
13							1	0	1	3	1	3	2	3	1	0	0	15
14								1	1	1	1	3	0	0	0	0	0	7
15									2	0	0	0	0	0	0	1	1	4
16										2	0	0	1	0	0	0	0	3
1											1	0	0	0	0	0	0	1
Fathers totals	2	45	63	0	78	91	6	8	94	96	99	105	85	63	12	1	6	1084
Offspring totals	3	60	135	252	325	450	419	458	618	607	581	10	5[illegible]	456	83	101	50	5928

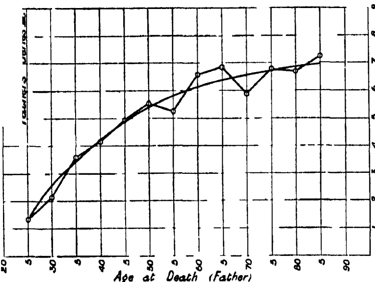
Table VIII.—Fathers and Offspring. Series IV.

Number of offspring	Age of father at birth: 25	30	35	40	45	50	55	60	65	70	75	80	85	90	95	100	105	Totals
0	2	5	6	2	10	11	7	4	6	6	7	7	6	2	1	0	0	82
1	2	3	5	1	4	4	3	8	6	5	8	1	1	2	0	0	0	53
2	1	4·5	4·5	5	5	5	4	5	3	11	[illegible]	6	4	0	0	0	0	67
3	1	3	7	8	7	[illegible]	10	[illegible]	14	10	11	13	6	3	0	0	0	103
4	0	2	5·5	7	6	8	13	8	15	12	19	15	5·5	4	0	0	0	121
5	1	2	3	6	9	15	10·5	17·5	16	15	10	7	9	1	0	0	0	131
6		1	1	6	9	[illegible]	11	[illegible]	14	13·5	[illegible]	5	8	4	0	0	1	109
7			1	1	3	4	4	[illegible]	12	13	6	13	1	4	0	0		72
8			1	3	6	5	3	11	14	15	8	13	2	4	0	0		85
9			1	[illegible]	1	1	8	[illegible]	7	3	2	12	4	1	1	0		51
10				0	4	4	[illegible]		3	7	7	5	5	1	0	0		49
11				[illegible]	0	1	4	3	2	3	1	6	0	0	0	0		22
12					0	2	1	5	4	[illegible]	5·5	0	1	1	1	0		23
13					0	[illegible]	0	2	0	2	[illegible]	4	0	0	0	0		10
14					1	0	[illegible]	1	3	1	0	1	1	0	0	1		9
15								0	0	0	0	2·5	0·5					3
16								1	1	[illegible]	0							[illegible]
17								1	1	0	0							2
18											0							0
19											0							0
20											1							1
Fathers' totals	7	20·5	35	41·5	65	76	81·5	108·5	121	122	122	114	54	27	3	1	1	1000
Offspring totals	12	45	102	184	290	357	[illegible]39·5	672·5	712	637	649	[illegible]13·5	284·5	147	21	14	6	5336

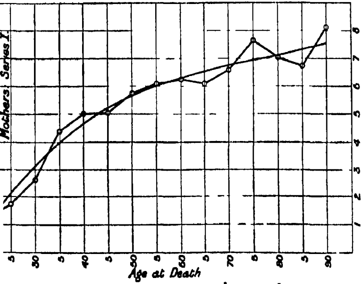
American Parents Regression Straight Lines (Figs 1 and 2)



American Parents *Regression Cubical Parabola* (Figs 3 and 4)

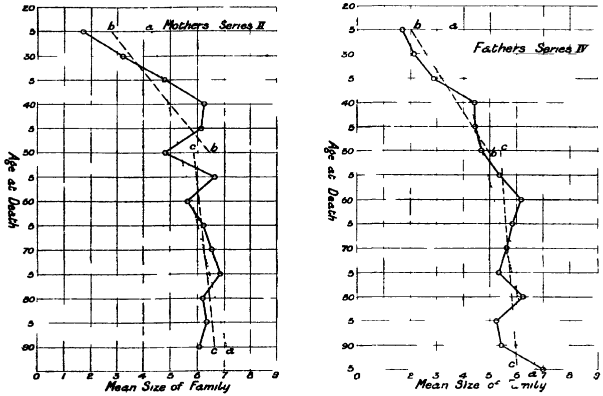


Fit with Cubical Parabola $y = 3\,8187 + \cdot363122x - \cdot047438x^2 + \cdot003035x^3$

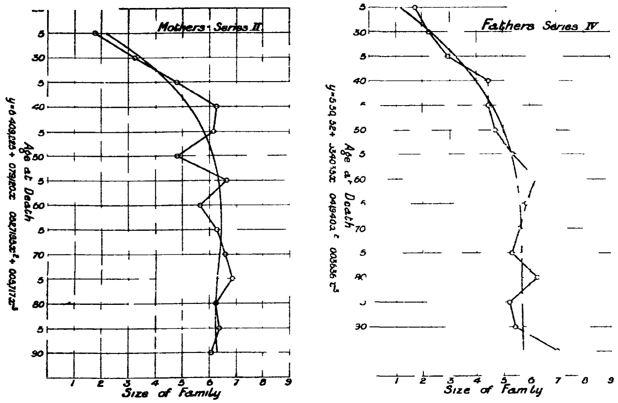


$y = 6\cdot020{,}731 + \cdot328{,}474x - \cdot033{,}056x^2 + \cdot003{,}000x^3$

Fighth Parental [illegible] Straight Line (Fig [illegible] and 6)



English Parental Regression Cubical Parabola (Figs. 7 and 8.)



"On the Effects of Changes of Temperature on the Elasticities and Internal Viscosity of Metal Wires."

By ANDREW GRAY, LL.D., F.R.S., Professor of Natural Philosophy in the University of Glasgow, and VINCENT J. BLYTH, M.A., and JAMES S. DUNLOP, M.A., B.Sc., Houldsworth Research Students in the University of Glasgow. Received May 24,—Read June 21, 1900.

At the outset the object of this investigation was to determine the variation, produced by rise of temperature, in the rigidity-modulus and in the Young's modulus of wires of different metals, but the observations made for this purpose yielded what seemed to us interesting information as to the viscosity of the substances examined, and led to an extension of the research. Heretofore but little attention seems to have been paid to alteration of rigidity with temperature, though several results, of apparently very different degrees of value, are available for Young's modulus. The earliest of these are probably those of Wertheim's experiments,* but on account of the smallness of the quantities observed it is not possible to regard them as even nearly correct.

Experiments were made about 1870 by F. Kohlrausch and F. E. Loomis.† After referring to the difficulties attending the method adopted by Wertheim, they remark: "All these difficulties disappear, however, and at the same time the most accurate method of observation is obtained, by employing for investigation the torsion elasticity, whose choice is further to be recommended from the fact that torsion is so generally employed in measurements. If a wire is loaded with a weight and set in vibration about its vertical axis, the reciprocal value of the square of the time of vibration affords a direct measure for the coefficient of the torsion of the wire. Since observations of the period of vibration are among the most accurate known in physics, the variations of elasticity may be thus determined with all the rigour desirable."

The authors seem here to indicate that the investigation of torsional elasticity would yield information to be compared with that obtained by Wertheim for Young's modulus, and this impression is confirmed by the remark which occurs later, that the results obtained "show no trace at all of the remarkable phenomenon of a maximum, alluded to at the beginning of this article, which would seem to be indicated for iron by the investigations of Wertheim." The Young's modulus is an essentially composite one, involving both the rigidity-modulus and the bulk-modulus, and it is impossible, except on strict experimental evidence, which has not so far been forthcoming, to suppose that there is any invariable numerical relation between the two latter moduli. Wertheim, it may be remarked, imagined that he had found evidence of an increase of the Young's modulus from 0 C to 100 C. and a diminution from 100 C. to 200° C. This result is negatived, as Messrs. Kohlrausch and Loomis notice, by the fact that if "two tuning forks are in vibration, and one of them is heated, the number of vibrations changes in the manner demanded by the assumption of a decrease of elasticity for increasing temperatures."

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* 'Annales de Chimie et de Physique,' tome 12, 1844.

† 'Pogg. Ann.,' bd. 141, 1870, or 'Amer. Jour. Sci.,' vol. 50, 1870.

It was found by Messrs. Macleod and Clarke,* in an experiment on the change of frequency of a tuning fork produced by alteration of temperature, that the period was increased by the fraction 11 × 10^{-5} for one degree of rise of temperature. The amount of this change due to expansion was very small in comparison with that due to diminution of the Young's modulus. For the linear expansion of steel is about 1·2 × 10^{-5} per degree, and as the period of a fork is altered in the direct ratio of the square roots of its corresponding linear dimensions before and after the expansion, if there is no change of modulus, the period was augmented by the expansion, by the fraction 0·6 × 10^{-5} for each degree rise of temperature. Hence the increase of period due to diminution of the Young's modulus was about 10·4 × 10^{-5} for each degree rise of temperature. And as the period is inversely as the square root of the Young's modulus, the fractional diminution of the Young's modulus must have been twice this amount or 20·8 × 10^{-5} for each degree rise of temperature. It will be seen below that the change of Young's modulus for mild steel is, according to our experiments, a diminution of about the same amount, though no doubt the change may be very different for different specimens of material.

In the 'Philosophical Magazine' for June, 1899, Mr. G. A. Shakespear has described an application of an interference method to the investigation of Young's modulus for wires, and has given values of the temperature changes found by measurements made in this way. Thus it was found that the final value of the Young's modulus was for copper, iron, steel, and hard brass lower at the higher temperature than that at the lower temperature by the respective percentages 3·6, 1·6, 3·2, and 3. Also it was noticed that repeated heating and cooling of the specimen seemed to produce an augmentation of the temperature-change of the modulus, the material apparently settling down to a steady state.

The method adopted by Mr. Shakespear was to elongate the specimen of wire (which was in each case about 28 cm. long and 0·75 mm. in diameter) by applying a weight of about 2 kilos. The elongation changed the difference of path of two rays from the same source, and so produced a change in the number of interference bands, which measured the retardation of one ray relatively to the other. The elongation was thus measured by the number of interference bands which passed across the field of view of the observing apparatus. The weight being kept hanging on the wire, the temperature was alternately raised and lowered a number of times until the number of bands measuring the change became approximately constant.

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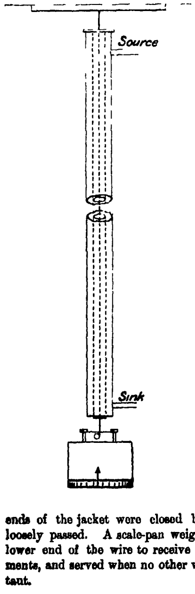
* 'Phil. Trans.,' vol. 171, Part I, 1880.

FIG. 1



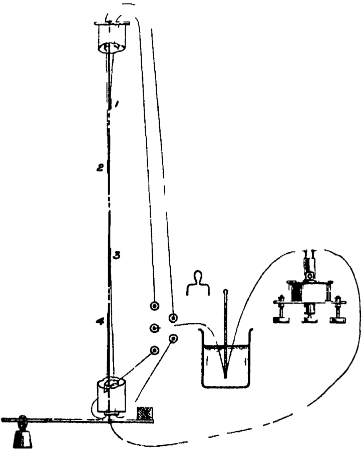
It was noticed that the first heating produced a *diminution* in the number of bands measuring the elongation, and the succeeding heatings an increase which finally settled down to a constant value.

In our experiments the method adopted was entirely different. The specimens were wires of radius varying from about 0·036 cm. to 0·06 cm., and each had a length of about 5 metres. The exact dimensions are given in the tables of results below. The experimental arrangement followed in the case of each wire was practically the same.

The wire was hung vertically in the laboratory from a plate at the top screwed firmly to the lower side of a wooden beam in the ceiling. The upper end was secured by being passed through a hole in the plate and firmly soldered at the back. The wire passed along the axis of a double-walled tube or jacket (fig. 1), consisting of two coaxial cylinders of tin-plate of diameters 3·5 cm. and 5 cm. respectively. The upper and lower ends of the jacket were closed by corks, through which the wire loosely passed. A scale-pan weighing about 4 lbs. was hung on the lower end of the wire to receive weights for the stretching experiments, and served when no other weight was applied to keep the wire taut.

Steam was generated under atmospheric pressure in an ordinary boiler and was led into the upper end of the space by what is called in the diagram the steam pipe while what remained uncondensed escaped by the sink pipe below It was found that the space in which the wire hung was thus heated to a fairly uniform temperature This temperature was determined by means of four thermo electric couples of copper and German silver the arrangement of which is shown in fig 2 A German silver wire passes along the axial space close to the

FIG 2



wire under test, and four copper wires are soldered to it at intervals as shown These copper wires are led out along the axial space in which the wire hangs Junction (1) is 25 cm from the top of the jacket, junction (2) 150 cm. lower down, junction (3) 150 cm still lower, and junction (4) 25 cm above the lower end of the jacket. An arrangement of mercury cups enables any of the thermal junctions to be brought into the circuit of the galvanometer and the cold junction. Care is of course taken to ensure that the wires leading from the junctions to the galvanometer are never in contact with the inner wall of the heating jacket or with the wire.

Each thermo-electric couple before use, and from time to time afterwards, was carefully calibrated in the usual way by comparison with thermometers placed with the junctions in water, the temperature of which could be conveniently altered through the range over which the experiments extended. The results were expressed in curves from which the temperatures of the junctions were obtained in the experiments. The sensitiveness of the galvanometer was such as to give about eight divisions deflection per degree of difference of temperature. This arrangement worked satisfactorily; but in future experiments it will probably be replaced by a platinum thermometer.

Determinations of Young's Modulus.

Experiments were made for each wire, first as to the variation of Young's modulus with temperature, next as to the variation of the rigidity modulus with temperature, and, in connection with this latter determination in each case, of the rate of dying out at the different temperatures of torsional oscillations The results of the last-mentioned observations are, we think, very interesting, but we give here in the first place the information obtained with regard to Young's modulus.

A scale-pan, weighing about 4 lbs., was attached to the wire and loaded to such an extent as not to produce gradual increase of length of the wire. A small reading-microscope, with a convenient scale in its eye-piece, was rigidly mounted on a heavily loaded table resting on the solid stone floor of the laboratory, and was focussed on the point of a very fine needle soldered to the wire just below the lower end of the heater. The needle was set so as to be as nearly as possible at the proper end of the scale to give a displacement along the scale when the weight was removed from the wire A similar arrangement at the upper end served to determine the amount by which the support yielded with different loads. This microscope was supported on a horizontal board firmly attached to two massive roof-girders at a distance from the beam to which the wire was suspended.

Enough of the load was then removed to cause the needle to traverse nearly the whole length of the scale, and the readings of the top and bottom microscopes were observed. A series of readings for weights on and off were first taken at the ordinary temperature of the room, and again after steam had been blown for some time through the heating jacket. The temperature of the internal space was observed before and after each series of readings, and in most cases also while the series of operations was in progress.

The distance between the needles was measured at the lower temperature; the distance at the higher temperature was obtained with sufficient accuracy from the difference of temperature and the result of a determination specially made of the coefficient of linear expansion of the wire. This coefficient was determined during the heating of the wire, and, as the change of length was beyond the range of the microscope scale, a scale was fixed behind the lower needle, and the increase of length read off on it by means of a telescope. The radius of the wire was determined after the wire was finally taken down by means of a micrometer from twenty readings taken at different points by each of two observers.

In two cases (wires II and III) the radius was determined by careful weighing of the wire in air and in water, and the results so obtained, properly corrected, agreed with those got by the micrometer method to within $\frac{1}{10}$ per cent.

Experiments were made for the following wires:—I, German silver; II, mild steel; III, brass; IV, copper (commercial); V, copper (hard drawn electrolytic); VI, soft iron. At first everything did not work quite smoothly, the arrangement of the microscopes required a little adjustment, and the determinations of moduli for I were somewhat doubtful in point of accuracy, and for II were certainly in error. The apparatus was, however, got into thorough order before the experiments on III were begun, and those on II were thereafter repeated. The following is the table of results for Young's modulus, in which is included the value obtained for German silver, notwithstanding the doubt as to its correctness, and the later value for mild steel, since there were good grounds for rejecting that obtained at first. The radii given in column VI were calculated for the higher temperatures from the expansions observed. The results of three separate determinations are given for the wire of soft iron (VI). Each elongation was obtained from the mean of about ten separate observations, agreeing to less than 0·5 of a microscope division. The elongations were obtained in microscope divisions, the value of which was 0·005948 cm.

The difference between the second and third values of the coefficient of diminution for soft iron and the first of the three values, depends upon 0·2 of a microscope division. The degree of accuracy to which the deflections could be read was to 0·1 of a division. The lower value, 0·000136, is probably more nearly correct than the higher, 0·000197.

Comparing the results with those obtained by Mr. Shakespear, reduced for comparison, we have for steel 0·000247 as against his 0·00038, for brass 0·000373 as against 0·000352, for copper (electro-

Wire	Weight applied in lbs	Length in cms	Elongation in microscope divisions	Coefficient of linear expansion	Radius in cms	Temperature	Modulus in 10^{1} dynes per sq cm
I German silver .	10	496 1	37 5		0 04915	20 3	1 3046
	,	496 316	38 5	0 00000654	0 04917	96 9	1 2701
II. Mild steel	12	496 6	45 6		0 03625	15 5	2 1279
	,	497 038	46 3	0 0000136	0 03828	90 3	2 1939
III Brass	4	498	36 1		0 03580	15 1	1 0257
	"	498 516	37 1	0 0000189	0 03584	90 1	0 9997
IV Copper, commercial .	10	493 182	35 7		0 05250	17 4	1 1192
	"	499 2	39 1	0 0000192	0 05258	92 3	1 1003
V. Copper, electro, hard drawn	12	498 218	30 9		0 05465	19 5	1 2954
	"	498 6	31 9	0 0000104	0 05970	93 2	1 2538
VI. (a)	16	498	35 9		0 05850	15 6	1 5450
	,	498 114	36 4	0 0000124	0 05856	93 9	1 5213
VI. (b)	"	498	35 6		0 05850	22	1 5578
	"	498 106	35 9		0 05856	95	1 5423
VI. (c)	"	498	35 7		0 05850	18	1 5536
	"	498 113	36 1		0 05856	95 8	1 5338

Coefficients of Diminution of Young's Modulus with Temperature.

Wire	Modulus	Difference	Difference of temperature	Coefficient of diminution of modulus per unit per degree C
I	1·3046	0·0345	66·6	0·000307
II	2·1279	0·0340	64·8	0·000247
III	1·0257	0·0287	70·8	0·000373
IV	1·1132	0·0129	74·9	0·000155
V	1·2054	0·0416	73·7	0·000436
VI (*a*) ...	1·545	0·0238	78·3	0·000197
(*b*) .	1·5578	0·0155	73·0	0·000136
(*c*) ...	1·5330	0·0108	77·8	0·000136

ytic) 0·000436 as against 0·000414 for "a specimen of copper," and 'or soft iron 0·000166 as against 0·000157, so that there is fair agree nent.

The difference between the coefficients for commercial copper (which was ordinary copper wire of the kind used by bell-hangers) and electrolytic copper is very great, but it is not more striking than the difference between the electric conductivities of the two materials

It was thought desirable to make a series of determinations for soft iron with the view of finding whether any progressive change due to alternate and repeated heating and cooling disclosed itself as in Mr. Shakespear's experiments. Nothing of the nature of change of sign (see Mr. Shakespear's paper) was observed, though there was some indication of a diminution of the coefficient.

Experiments on the Rigidity Modulus, and Observations on the Internal Viscosity of the Wires.

Experiments were made on each wire, as has been stated above, to determine the change of rigidity produced by the alteration of temperature. These experiments were made by the torsional oscillation method. Cylindrical vibrators were attached to the wires such that the moment of inertia of the vibrating system was practically that of the cylinder only; and observations were made for each wire with two separate vibrators of different weights, except in the cases of German silver and steel, for which one vibrator only was used, and electrolytic copper, for which four vibrators were used.

The length of wire used was, with the vibrator attached, about 505 cm. in each case. The lengths and radii at the different temperatures were calculated from the expansions as in the Young's modulus experiments. The results of the experiments are given in the following table;—

Wire.	Weight of vibrator	Period	Temp.	Modulus in 10^{11} dynes per sq cm
I German silver	12·7	27·19	20·7	3·4256
	"	27·65	88·3	3·3033
II Mild steel	18·7	35·71	17·0	7·9648
	"	36·12	89·2	7·7704
III Brass	12·7	50·29	15·4	3·5519
	"	50·71	87·7	3·4913
	3·4	26·58	16·1	3·4066
	"	26·9	87·0	3·3214
IV Copper, commercial......	12·7	20·81	19·3	4·4822
	"	21·07	92·4	4·3549
	18·7	25·64	19·1	4·3440
	"	25·98	92·0	4·2230
V Copper, hard drawn, electrolytic	12·7	16·56	23·0	4·2396
	"	17·20	90·7	3·9402
	18·7	20·25	22·5	4·1908
	"	21·33	90·1	3·7602
	24·7	23·23	10·8	4·1994
	"	24·6	89·8	3·7305
VI Soft iron	12·0	12·33	13·6	8·2855
	"	12·52	93·8	8·0125
	18·0	15·13	15·6	8·0915
	"	15·44	93·8	7·7501
	24·0	17·23	16·0	8·2800
	"	17·46	95·1	7·9903

The following table shows the coefficient of diminution of the rigidity modulus as deduced from the results given in the last table.—

Wire	Vibrator.	Difference of temperature	Coefficient of diminution of rigidity per unit per degree C
	lbs		
I..............	12·7	67·6	0·000528
II..............	18·7	72·2	0·000338
III	3·4	70·9	0·000352
	12·7	72·3	0·000247
IV...	12·7	73·1	0·000392
	18·7	72·9	0·000418
V..............	12·7	67·7	0·00111
	18·7	67·6	0·00149
	24·7	70·0	0·00160
VI..............	12·7	80·3	0·00041
	18·7	78·2	0·00064
	24·7	79·1	0·00087

With respect to this table, it is to be observed that the results for soft iron and brass agree fairly with those obtained by Kohlrausch and Loomis for those metals, which were coefficients of diminution

0·000447 and 0·000428 respectively. The result given above for commercial copper agrees also moderately well with that obtained by these experimenters, viz., 0·000520. But here, as indeed in all the substances examined, the differences in results are not surprising, in view of the probable difference of composition of the specimens.

The great excess of the coefficient in the case of hard-drawn pure copper above the value for commercial copper is very noteworthy. It will be interesting to examine, as we propose to do immediately, the behaviour of copper wire soft drawn as well as hard, and to trace the effect, if any, of repeated heating and cooling.

It was noticed that the rate of dying out of the torsional oscillations was very different at the different temperatures, being, except in one case, that of German silver, very much more rapid at the higher temperature than at the lower. This is undoubtedly the effect of a considerable increase of the internal viscosity of the wire with rise of temperature, for no practical difference existed in the immediate surroundings of the vibrator, which of course was the part mainly affected by the air in the vibrations. The top of the cylindrical vibrator in each case was about 8 cm. below the lower end of the heater, and the length of the cylinders, which were of brass, varied from 7 to 13 cm. There was no appreciable change of the air-temperature at the vibrator produced by the heater.

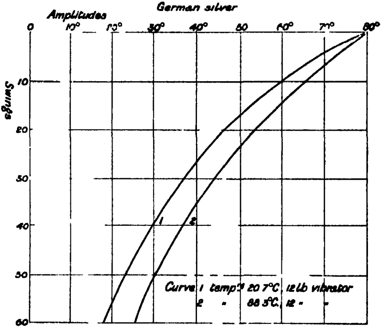
This difference in the rates of subsidence of the torsional oscillations seems to us very remarkable, and, so far as we know, has not been observed before. It is shown in the diagrams numbered I—VI.

[*Note added June* 20, 1900.—The change in rate of subsidence produced by alteration of temperature had, we have since found, been observed by Streintz and by Pisati ('Pogg. Ann.,' 153, 1874, and 'Gazzetta Chimica Italiana,' 1876, 1877, also 'Sitzungsb. d. Wien. Akad.,' lxxx, Abth. 2, 1879). In the first and last papers here cited, Streintz gives an account of his own work and of that of Pisati, principally as bearing on the effect which he called "Accommodation." In certain wires, *e.g.*, steel, copper, silver, brass, and platinum, examined by Streintz and Pisati, a marked diminution of rate of subsidence was produced by keeping the wire for a considerable time in continual torsional oscillation. This is contrary to results obtained by Lord Kelvin at an earlier date, 1864-5 (Art. "Elasticity," 'Collected Papers,' vol. III).

Since learning of the investigations here referred to, we have begun to extend our experiments to the question of "fatigue of elasticity," and have found that the rate of subsidence appears to be, in some cases at least, a function of the temperature and of the amplitude of vibration. It seems possible thus to reconcile the discordance of results thus referred to, and we hope to make a communication on the subject at an early date.]

In each curve the abscissæ are amplitudes of vibration in degrees and the ordinates which are drawn downwards in each case, are the numbers of swings that have been completed from the instant at which the amplitude was 80 the largest amplitude of swing used in the observations of periods The temperatures are marked on each diagram for the several curves Curves (1) and (2) of diagram (I) represent

Diagram I

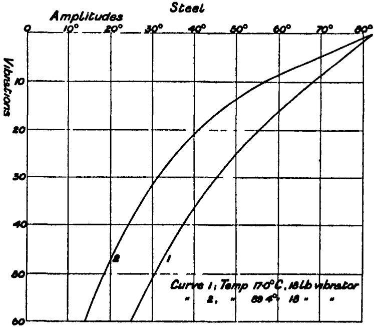


the observations made in German silver at the lower and higher temperatures respectively stated in the diagram It will be seen that the rate of dying out of the amplitude is distinctly faster at the lower than at the higher temperature The reverse was found to be very markedly the case in all the other wires examined

In mild steel at 17 C the oscillation as shown by the curves of diagram II has fallen from 80 to about 55 in 20 periods, while at 89 4 C the amplitude has fallen to 40 in the same time After 60 periods the amplitudes were respectively 25 and 13 The logarithmic decrement is practically constant during the subsidence at the lower temperature At the higher temperature the logarithmic decrement diminishes in the ratio of about 75 to 60

In brass, for which the different vibrators weighing 3 4 lbs and 12 7 lbs respectively were employed, the rate of subsidence was determined at the two temperatures (16·1° C. and 87° C., and 15·4° C. and 87·7° C), first with the lighter vibrator, then with the heavier.

DIAGRAM II.



Curves (1) and (2) of diagram III give the the first two sets of results, (3) and (4) the second. It will be seen that the subsidence with the heavier vibrator was more rapid at each temperature than with the lighter vibrator. The logarithmic decrements are practically constant throughout each of the two curves.

A question arises here, which further experiments are being made to answer for the different wires, as to the effect of repeated heatings and coolings on the rate of dying out at any one temperature? Some effects of this kind appear to be visible in the results obtained for one or two of the other wires experimented on.

Three pairs of curves are given in diagram IV(*a*) for commercial copper, and were obtained with a 3·4 lb., a 12·7 lb., and an 18·7 lb. vibrator respectively, and in this order. Here it will be seen that the rate of subsidence is greater with each vibrator in the order of its weight; that is, the larger weight in each case corresponds to a greater rate of subsidence, whether the temperature be the higher or lower of those used in the experiments. Also the rate of sub-

DIAGRAM III

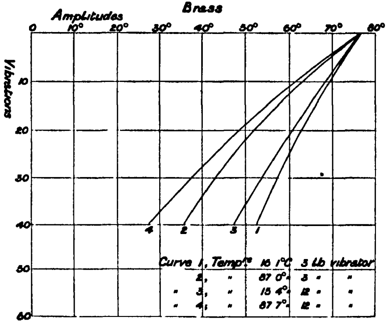
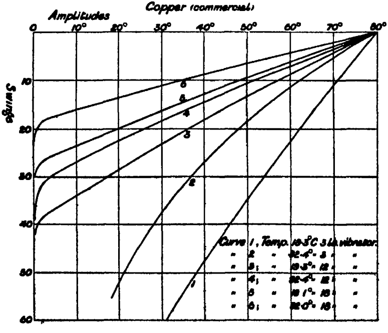


DIAGRAM IV(a)



sidence is in each case greater at the higher temperature than at the lower.

It was observed by Lord Kelvin that increase of period as well as increase of stretching force affected the rate of subsidence, these effects being opposite. The effect of increase of stretching force increased the rate of subsidence. The effects of increased mass of vibrator given here are therefore mainly those due to increase of pull in the wire. We are, however, arranging to alter the period without altering the pull, and to alter the pull without changing the period, by using suitable vibrators.

There is one characteristic of these curves for commercial copper which was shown also in the case of soft iron, but which does not appear in the other experimental results. The falling off of amplitude goes on very quickly in curves (3) and (4), and (5) and (6) in such a way that the curves are almost straight lines, until the amplitude has come down to only some 3 or 4 degrees, and then the subsidence becomes comparatively slow. The curves for the 3 lb. vibrator show at the low temperature a logarithmic decrement increasing in the ratio 27 to 60 as the amplitude falls from 80° to 24°. At the higher temperature the logarithmic decrement is practically constant.

Diagram IV(*b*)

Copper (commercial).

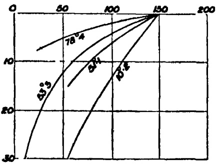


Diagram IV(*b*) shows subsidence at four temperatures which, taken in the order in which the experiments were made, were 10·2°, 51·1°, 53·3°, 78·4° C. In this diagram 150 divisions of abscissa represent 34°.

Diagram V(*a*) gives the rates of subsidence for pure electrolytic copper at temperatures 23·2° and 92·1° with the 3 lb. vibrator. Here there is no sign of the change of character of the curves of subsidence at low amplitudes which has just been noticed. The curve of subsidence at the higher temperature is indeed nearly an exponential curve at all amplitudes less than 20°. The curve for the lower temperature shows a logarithmic decrement at first somewhat quickly, diminishing then more slowly. The initial and final values are in the ratio of 73 to 44.

Diagrams V(b), V(c), V(d) are the curves of subsidence at two temperatures—for the 12·7 lb. vibrator, the 18·7 lb vibrator, and the 24·7 lb. vibrator respectively. It will be observed that the curves of subsidence are nearly alike in the two sets V(b) and V(c), only a very slight difference in the direction of faster subsidence with the heavier vibrator being visible. The curves of subsidence at the higher temperature with the 24 7 lb. vibrator show distinctly more rapid subsidence at the lower temperature than in any of the other three cases.

Diagram VI shows the curves of subsidence for soft iron. Curves (1) and (2) were obtained with the 12·7 lb. vibrator; the former curve gives the subsidence at the lower temperature, the latter at the higher temperature stated in the diagram. It will be observed that the rate of subsidence at the higher temperature is very much greater than at the lower. These two sets of experiments were made in direct succession to one another on May 15.

Next, on May 16, two sets of experiments at temperatures 15°·6 and 93°·8 C. were made with the 18·7 lb. vibrator. The results of these are shown in curves (3) and (4). Between these curves there is a much smaller difference than between (1) and (2), though the temperatures were very nearly the same.

The high-temperature experiments were repeated on both days with reproduction of practically the same curve.

After twenty-four hours nearly had elapsed the experiments at the lower temperature were repeated with the same vibrator, when the rate of subsidence was found to be much slower at 13°·5 C. than it had been on the previous day at 15°·6 C.

On May 18 two curves (5) and (6) were obtained with the 24·7 lb. vibrator at the respective temperatures 16° C. and 95°·1 C. Curve (6) does not differ very widely from the curve (4) obtained on May 16 with the 18 7 lb. vibrator; but the curve at the lower temperature lies much to the right of that (3) obtained on May 16 at the lower temperature, the companion curve of (4).

All the results in curves (3)—(6) show rates of subsidence lying between those shown in (1) and (2). Curve (1) is approximately exponential. It will be observed also that curves (2) and (4) show the somewhat rapid change of direction when the amplitudes have become small to which attention is directed above in connection with commercial copper. Curve (6), however, obtained at 95·1° C. with the 24·7 lb. vibrator, shows no such change of direction; but a considerable part of its curve, after the amplitude has been reduced to about 10° C., is approximately exponential in character. All the curves except (1) and (4), however, show very marked diminution of rate of subsidence at the lower amplitudes. It seems certain that

DIAGRAM V(*a*)

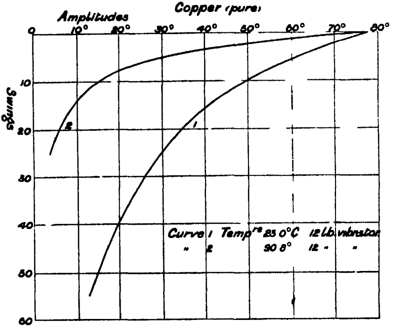


DIAGRAM V(*b*)

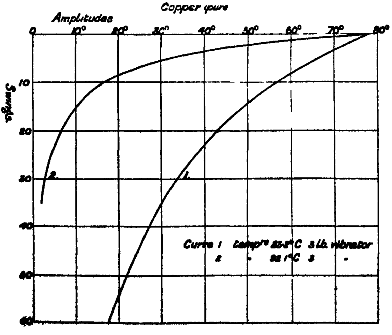


DIAGRAM V(c)

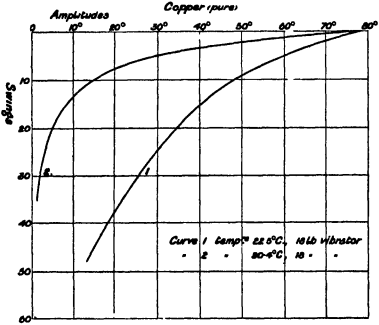


DIAGRAM V (d)

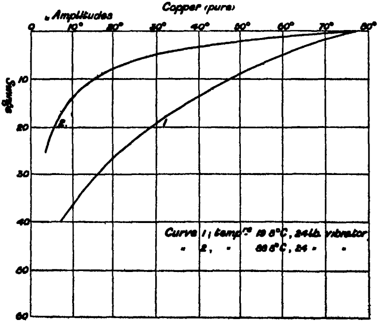
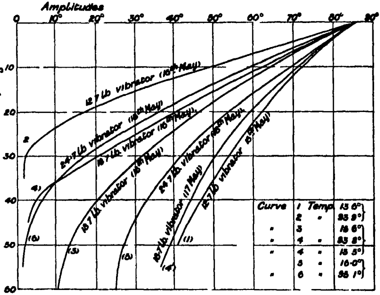


DIAGRAM VI —Soft iron



the successive operations have had a considerable effect on the behaviour of the wire and experiments are being made to elucidate this effect as far as possible

Some other points of interest which were observed in these experiments are reserved for further investigation in a continuation of the work now in progress

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On the Connection between the Electrical Properties and the Chemical Composition of Different Kinds of Glass Part II"

By Professor ANDREW GRAY LL D F R S and Professor JAMES J DOBBIE M A D Sc

Received May 25—Read June 21 1900

In a former paper* we described experiments on the electrical qualities of specimens of glass of which the chemical composition was determined by analysis Results were given for a lead potash glass made by Messrs Powell and Sons, of London, a lead potash glass made by Messrs Schott and Co , of Jena, a barium glass, and a zinc-soda glass ("Jena glass"), both made by Messrs Schott and Co These specimens are referred to in the paper by the numbers XXI—XXIV.

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* 'Roy. Soc. Proc ,' No 390, April 20, 1898

We have now to communicate the results of some further experiments, the object of which was to throw light on various points which had arisen in connection with the previous experiments, and to afford information as to whether the resistance or capacity of the glass was affected by the process of annealing, or varied with time. The specimens here referred to are numbered XXV—XXXII, and were all made, as nearly as possible, according to a previously prescribed composition, and in the form of flasks, with long thick-walled necks, adapted for experiments by the direct-deflection method formerly employed. Full particulars of the different specimens are given in Table I below.

The method of experimenting and the method employed were the same as those described in our former paper. The chief sources of error which had to be guarded against were, as before, surface conduction, due to moisture on the surface of the glass, and leakage at other parts of the apparatus, due to the want of perfect insulation. The most careful watch was kept throughout the experiments against the possibility of inaccuracy from these causes, and tests were made in connection with each determination to make sure that everything was working correctly.

The Resistance Experiments.

Only a few days before the meeting of the Royal Society at which the paper referred to above was read, a rough test was made of the resistance of a flask (XXVII below) made for us by Messrs. Powell and Sons, which had approximately the same composition as Specimen XXI, the potash, however, being replaced by soda. It will be seen by a reference to our former table of results, that XXI was a lead-potash glass of very high specific resistance, certainly above 18000×10^{10} at 130° C. It was anticipated from our experiments that the substitution of soda for the potash in this glass would very greatly diminish the specific resistance, and it was stated when the paper was read that this conclusion had been verified. More accurate determinations made since that time have confirmed this result, as will be seen by a comparison of Table I with the table given in the former paper. While XXI had the resistance at about 130° C. quoted above, the specific resistance of XXVII at about the same temperature was only 136×10^{10}; so that the substitution of soda for potash in the composition of the glass diminishes the resistance of the glass to $\frac{1}{130}$ of its former amount.

The influence of the substitution of soda for potash is still more clearly brought out by a comparison of XXIX with XXXI, and XXX with XXXII. Specimens XXIX and XXX are lead-potash glasses, XXX and XXXII lead-soda glasses, in which the amount of soda is nearly equivalent chemically to the potash in XXIX and XXX respectively. Reference to Table I will show that in both cases the potash glasses have very much higher resistances than the soda glasses.

Both XXI and XXVII were lead glasses; but in the previous paper a glass was discussed which was made by Messrs. Schott and Co., and was composed mainly of barium oxide and alumina in combination with silica and boron trioxide. This, which we shall call the Jena barium glass, had a very high resistance, one quite unmeasurable, indeed, within the range of temperatures covered by the experiments. Moreover, it was found that the inductive capacity in a plate of this glass was exceedingly low, and that the glass showed little or no effects of dielectric polarisation.

It was thought that it might be of interest to find whether the high resistance of this glass was associated with the presence of the large percentage of barium oxide. Accordingly, further experiments have been made on flasks of barium-potash glass manufactured by Messrs. Powell and Sons. This glass is numbered XXVIII of Table I. Its resistance is very low in comparison with that of either the lead-potash glass or the Jena barium glass, which, however, it must be remembered contained no potash. It was found to be subject to a somewhat rapid disintegration of its surface, and it probably differed in physical constitution from the Jena glass. The Jena glass, moreover, contains a considerable quantity of boron trioxide and alumina, which are absent from the glass made by Messrs. Powell. The presence in glass, however, of a considerable percentage of potash with lead is, as shown by XXIX and XXX, consistent with a high specific resistance.

It is interesting also to compare XXVIII and XXVI, which contain approximately the same percentage of barium and lead oxides respectively, and are otherwise very similar. The lead glass, XXVI, contains soda, from which the barium glass, XXVIII, is free; but in spite of this, the resistance of XXVI is about three times that of XXVIII at the same temperature.

Comparison with Ordinary Glass.—Experiments were next made to test how the glasses of specially prescribed chemical composition, already experimented on, compared with the ordinary kinds of glass used in the construction of apparatus. A common lime glass, XXV, and common lead glass, XXVI, were examined. The resistances of both of these were low; that of XXV was low in comparison with the resistance of any other glass in the table.* As has been pointed out, however, the resistance of XXVI considerably exceeded that of the barium glass, XXVIII.

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* It may be noticed here that the first experiments on the electrical properties of specimens of glass, which were afterwards subjected to chemical analysis, seem to be those described in Mr. T. Gray's paper, 'Roy. Soc. Proc.,' vol. 34, p. 199. The analyses of the specimens (which were of glasses used for different purposes in the arts) were made with great care by Professor Divers, of Tokio.

Effect of Annealing.—The last four specimens mentioned in Table I (XXIX—XXXII) were experimented on to find the effect of annealing on the resistance of glass. It is well known that in most conductors, especially pure metals, the effect of annealing is to diminish the specific resistance. XXIX and XXX were specimens of Messrs. Powell's lead-potash glass. Of these, XXIX and XXXI were carefully annealed in the usual way by Messrs. Powell: XXX and XXXII were left in the unannealed state.

The results are given side by side in Table I, and show that the effect of annealing glass is very greatly to increase its specific resistance. In the case of XXX and XXXII, the lead-soda glasses, the specific resistance has been raised to three times its former value. Annealed glass is therefore a much better insulator than unannealed glass.

Variation of Resistance with Time.—The question of variation of resistance with time has been investigated by testing flasks, which had been set apart for the purpose, at intervals of about six months. The results are shown in Table II. XXI, XXVII, and XXVIII have had their resistances determined three times, and so far no change has disclosed itself. As noticed above, the surface of the glass XXVIII seems to become disintegrated in course of time; for the surface, which was originally cleaned till quite clear, has gradually acquired a milky appearance, and is now quite opaque.

The Capacity Experiments.

The specific inductive capacity of the glass of these flasks was determined by the method described in our former paper, and the results are shown in Table I. It will be seen that the specific inductive capacity of Powell's lead-soda glass, XXVII, is rather low in comparison with that of the corresponding lead-potash glass, XXI, which was about 8.

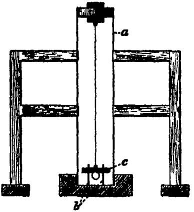
It is noteworthy, however, that the lead-soda glass was free from the dielectric polarisation effects which were so troublesome in the case of XXI. Of the glasses discussed in the present paper, XXVI, one of the ordinary glasses experimented on, was the only one that showed dielectric polarisation conspicuously.

No trouble from dielectric polarisation was experienced with Nos. XXIX—XXXII, so that annealing does not cause any marked differences in the "electric absorption" of the glass.

Measurement of Residual Twist of Glass Fibres.

The object of these experiments was to compare fibres of the glasses, on which the electrical experiments already described had been carried out, as regards imperfection of their torsional elasticity.

The figure shows a sketch of the apparatus used. A long cylindrical glass tube (*a*), about 23 inches in length and 2 inches in diameter, was fixed permanently in a vertical position in a wooden frame, as indicated in the diagram. A tightly fitting cork closed the upper end of the tube, and through a narrow slit in this passed a rectangular piece of copper foil, from the centre of the lower edge of which projected a short tag of the foil. This tag was quite rigid, and carried attached to it with shellac cement the glass fibre, care being taken that the fibre was so placed that it hung vertically along the axis of the tube, when the cork was fixed in position. The lower end of the fibre was attached in a similar manner to a cross-piece, *c*, weighing about 1 gramme, and having a length of about 1¾ inches. The fibre was attached to the upper end of the cross, and to the lower end was fixed a small mirror.



The lower end of the glass tube fitted into a groove cut in a wooden sole-plate, capable of being rotated about a vertical axis, which coincided as nearly as possible with the axis of the tube, that is, with the position of the fibre. Two brass pins (*b*) were fixed vertically at two points in the sole-plate within the tube, and, projecting upwards, stood one on one side the other on the other side of the horizontal arm of the cross-piece. By turning the sole-plate round, any required twist could be given to the fibre, since the tube was held fast in its supporting frame.

A lamp and scale set in front of the mirror enabled the position of the lower end of the fibre to be observed.

After fixing the fibre and allowing the arrangement to stand undisturbed for a day or two to allow the cement to harden, the scale was placed in position with its ends at equal distances from the mirror. The zero position of the spot of light on the scale was observed, and the sole-plate turned round to give a total turning of the lower end of the fibre relatively to the upper of 360°. The couple was kept applied for thirty seconds, and then taken off gradually without any jerking of the fibre. This prevented torsional oscillation of the fibre after the removal of the couple. In fifteen seconds after the removal of the couple a reading of the position of the spot of light was taken. From this the angular deflection of the lower end of the fibre was obtained, and the angle so measured, divided by the length of the fibre, gave the residual twist.

The following table gives the results in radians per centimetre of the length of the fibre × 10^4. The diameter of the fibre was found by weighing a known length of it, and calculating from the known density of the glass.

No. of specimen.	Diameter in centimetres.	Residual twist.
XXI	0·0109	2·09
XXII	0·016	1·84
XXIII	0·010	0·93
XXIV	0·0105	3·83
XXV	0·0115	7·19
XXVI	0·015	5·41
XXVII	0·0187	2·44
XXVIII	0·0105	5·52
XXIX	0·0142	0·83
XXX	0·0183	0·8
XXXI	0·0148	1·9
XXXII	0·0191	2·8

The experiments on Specimens XXI—XXVII inclusive were made in June and July, 1898, and it was then thought that there appeared to be some connection between the resistances and the residual twist of the fibres. But since XXI to XXIII were glasses whose resistances were too high for measurement, it was of course impossible to draw any numerical conclusion on the point without examining more specimens. A very little reflection, however, showed that no exact comparison was possible from this point of view, as the residual torsion was no doubt influenced in a very marked degree by the immediate previous history of the fibre. But it is noteworthy that the residual twist is very low in the case of the Jena barium glass and in Messrs. Powell's lead-potash glasses, viz., in XXIII, XXIX, XXX, and is abnormally high for the two ordinary glasses, XXV, XXVI

It was found that for the same fibre the same twisting couple produced the same residual twist, provided the two determinations were not made in immediate succession. The rate at which the residual twist came out was very great immediately after the twisting couple had been taken off; and this rate diminished rapidly as the spot of light approached its zero position on the scale.

Chemical Composition of the Specimens of Glass.

The results of the chemical analyses made for the different specimens are stated briefly in Table I, which affords a conspectus of all the results of the experiments now described.

The following notes regarding the different specimens, containing approximate empirical formulæ for their composition, are, however, set down here.

XXV. This is an ordinary lime glass. The alkalies were not estimated separately, and no formula can therefore be given for it.

XXVI. This is an ordinary lead-alkali glass, containing considerable quantities both of potash and soda. After deducting ferric oxide, alumina, and manganese, its composition may be represented by the empirical formula

$$43SiO_2, 5PbO, 5Na_2O, 3K_2O.$$

	Found.	Calculated.
SiO_2 ……………	59·60	60·18
PbO . …. ……	26·44	26·00
Na_2O …………	7·44	7·23
K_2O . …………	6·50	6·57
	99·98	99·98

XXVII. This glass is composed of silica, lead oxide, and sodium oxide, and is free from potassium. Allowing for the small amount of ferric oxide and alumina which it contains, its composition may be expressed by the empirical formula

$$10SiO_2, 3PbO, 3Na_2O.$$

	Found.	Calculated.
SiO_2 ……………	40·87	41·24
PbO ……………	45·33	45·98
Na_2O……………	13·80	12·78
	100	100

XXVIII. This is a barium-potash glass, free from lead, and containing only a very small amount of soda. After allowing for the

Table I.

Number of specimen.	Description of glass.	Density.	Dimensions. d = thickness of bulb. s = effective surface. r = external radius.	Resistance.	
				Temperature, centigrade.	Specific resistance in ohms $\times 10^{10}$.
XXV	Lime glass.........	2·487	d = 0·128 cm. s = 174·614 sq. cm. r = 3·829 cm	149° 116 93 72 55	0·202 1·874 11·901 89·15 531·05
XXVI	Lead glass, with potash and soda	2·99	d = 0·0668 cm. s = 188·19 sq. cm r = 3·971 cm.	150° 140 130 101 88 66	8·535 18·64 33·59 442·70 1956·50 18034·0
XXVII	Lead-soda glass made by Messrs Powell and Sons, London	3·552	d = 0·092 cm. s = 211·54 sq. cm. r = 4·226 cm	142° 116 90	136·5 797·3 5249·0*
XXVIII	Barium-potash glass made by Messrs Powell and Sons	3·11	d = 0·127 cm. s = 240·1 sq. cm. r = 4·566 cm.	138° 125 95 77	6·47 16·01 178·05 1115·50
XXIX	Lead-potash glass made by Messrs. Powell and Sons (annealed)	3·41	d = 0·086 cm. s = 295·411 sq. cm. r = 4·422 cm.		Resistance too high to measure. Certainly above 29000 at 140°
XXX	Lead-potash glass made by Messrs. Powell and Sons (unannealed)	3·34	d = 0·906 cm. s = 211·286 sq. cm. r = 4·2014 cm.	142°	1328·6 Too high to measure at lower temperatures
XXXI	Lead-soda glass made by Messrs Powell and Sons (annealed)	3·408	d = 0·059 cm. s = 226·705 sq. cm. r = 4·3109 cm.	141° 122 102 84	4·874 20·497 102·820 515·94
XXXII	Lead-soda glass made by Messrs. Powell and Sons (unannealed)	3·36	d = 0·0985 cm. s = 213·246 sq. cm. r = 4·1921 cm.	140° 120 104 88 78	1·691 4·227 20·821 144·640 215·180

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* This result at 90° is not accurate, but

Table I

Capacity		Chemical Composition							
Temperature centigrade	Specific inductive capacity	Silica	Lead oxide	Barium oxide	Calcium oxide	Potassium oxide	Sodium oxide	Ferric oxide and alumina	Manganese (calculated to MnO)
11° 129	6 20 6 79	69 04 —	very small trace		7 59	19 48		2 73	1 81
10° 130	7 06 7 30	58 82	26 10		trace	6 42	7 35	0 43	0 81
8 130	5 42 5 69	40 75	45 19				13 76	0 29	
22° 147	6 93 7 18	52 28		26 09		19 74	0 64	0 45	
18 140	7 22 7 42	48 25	40 80			10 65		0 29	
18 140	6 76 7 05	50 11	39 74			10 08		0 12	
20° 140	8 013 8 302	50 42	40 24			trace	8 77	0 88	
19° 180	7 376 8 44	51 46	38 94				9 13	0 45	

serves to show the order of the quantity

Table II

Number of specimen	Dimensions d = thickness of bulb s = effective surface	Date of test	Temperature centigrade	Resistance Specific resistance in ohms × 10
XXI		April 27 1898	Up to 110°	Resistance too high to measure
		Dec 16 July 3 1899	„	
XXVII	t = 0 085 cm s = 215 15 sq cm	Mar 31 1898	138	106 17
			107	1609 3
			90	Resistance too high to measure
		Mar 19 1898	142	73 8
			111	505 85
			90	Resistance too high to measure
		July 4 1899	144	72 28
			115	585 9
			90	Resistance too high to measure
XXVIII	d = 0 158 cm s = 230 686 sq cm	June 7 1898	145°	2 37
			114	24 32
			93	152 79
			72	1039 25
		Jan 13 1899	130	9 35
			106	61 77
			89	296 97
		July 5 1899	142	5 045
			105	91 56
			85	461 64
XXIX	d = 0 125 cm s = 204 306 sq cm	July 6 1899	Up to 140°	Resistance too high to measure
XXX	t = 0 1185 cm s = 210 14 sq cm	July 7 1899	140°	1777 5 Could not be measured at lower temperature
XXXI	d = 0 0483 cm s = 219 5 sq cm	July 11 1899	136°	7 539
			100	102 04
			80	316 40
XXXII	d = 0 1438 cm s = 208 8 sq cm	July 10, 1899	141°	0 8502
			110	8 175
			69	298 95

small quantities of ferric oxide, alumina, and sodium oxide, its composition may be represented by the formula

$$20SiO_2,\ 4BaO,\ 5K_2O.$$

	Found	Calculated.
SiO_2 . .	53·29	52·54
BaO	26·59	26·87
K_2O	20·12	20·58
	100	99·99

XXIX and XXX. Both of these glasses are from the same pot, XXIX being annealed, and XXX unannealed. Although they yielded slightly different numbers on analysis, they are essentially the same, and may be represented by one formula, viz.,

$$23SiO_2,\ 5PbO,\ 3K_2O.$$

	Found		
	XXIX	XXX	Calculated.
SiO_2	48·39	50·17	49·69
PbO .	40·92	39·78	40·15
K_2O . .	10·68	10·04	10·15
	99·99	99·99	99·99

XXXI and XXXII. These two glasses correspond closely to XXIX and XXX, soda, however, being substituted for potash. It was hoped, in preparing them, to obtain a glass closely corresponding to XXIX and XXX, soda being substituted for potash in exactly equivalent quantity; but Messrs. Powell found that in order to make the glass workable, it was necessary to add a slight excess of soda. XXXI was annealed; XXXII remained unannealed. The composition of both may be expressed by the formula

$$24SiO_2,\ 5PbO,\ 4Na_2O.$$

	Found.		
	XXXI.	XXXII	Calculated.
SiO_2	50·70	51·7	51·37
PbO	40·47	39·12	39·77
Na_2O	8·82	9·17	8·84
	99·99	99·99	99·98

We desire, in conclusion, to express our thanks to Messrs. Powel and Sons for their kindness in preparing glasses for us, and to Messrs. O. W. Griffith and Robert Abell for their help in the electrical and chemical experiments respectively.

"On the Change of Resistance in Iron produced by Magnetisation."

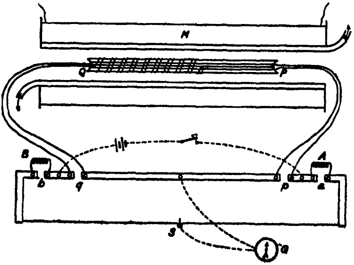
By ANDREW GRAY, LL.D., F.R.S., Professor of Natural Philosophy in the University of Glasgow, and EDWARD TAYLOR JONES, D.Sc., Professor of Physics in the University College of North Wales. Received May 30,—Read June 21, 1900.

The experiments described below were made with the object of determining simultaneous values in a specimen of soft iron wire of the magnetising force, the magnetisation, and the change of resistance due to magnetisation. In all the measurements hitherto made of magnetic changes of resistance no attempt seems to have been made to determine at the same time the magnetisation; in fact, the results which have been obtained for bismuth might appear to indicate that there is but little, if any, connection between magnetisation and change of resistance. The results herein described, on the other hand, indicate that there is a somewhat close connection between the two phenomena, and make it clear that further measurements on similar lines would have some value.

Measurement of Resistance.

Preliminary trials showed that great difficulty would be experienced in determining the magnetic change of resistance in iron unless great precautions were taken to eliminate the effects of rise of temperature in the wires due to the passage of even feeble currents. In the arrangement finally adopted, *two* coils of soft iron wire P, Q (fig. 1)

FIG. 1.



were used, of nearly equal lengths, cut from the same specimen, and doubly wound with silk. One of these was wound longitudinally, the other spirally and non-inductively, on a rod of wood about 60 cm. long. The whole was placed inside a magnetising coil, so that one coil, P, became longitudinally, the other, Q, transversely magnetised.

On account of the great demagnetising factor for a cylinder magnetised transversely, however, the latter coil was only very feebly magnetised. The magnetising coil was 1 metre long, and was provided with a double cylindrical core through which a constant stream of water could be kept flowing, in order to diminish the heating effect of the magnetising current. It was further arranged in the experiments that equal currents should flow in the two iron coils, which were thus subjected to very approximately equal rises of temperature. These arrangements, though not perfect, so much diminished and retarded the heating effects of currents in the various coils that these could be readily distinguished from the magnetic effect.

The comparison of the resistances of the coils P, Q was carried out in the usual way. The platinoid wire of the bridge was replaced by one of somewhat thick copper, so as to give an easily measurable step of the slider for the small alteration of the ratio of resistances of the coils which had to be measured, the iron coils inserted in p, q (fig. 1), and two nearly equal coils A, B in a, b. A and B were both kept in one bath of oil. All connecting wires were of thick copper. The galvanometer was a Kelvin low-resistance astatic instrument. The magnetising current was measured by a Kelvin graded galvanometer, standardised by electrolysis and by comparison with a Kelvin deciampere balance.

In making observations, the iron coils P, Q were first demagnetised by reversals, and the position of the sliding contact key S found, which gave no galvanometer deflection.

Then a weak magnetising current was applied, reversed several times, and the position of S again found. The change of position of S indicated that the resistance of the longitudinally magnetised wire became greater than that of the transversely magnetised wire. This was repeated for a large number of field-strengths, each greater than the one preceding it. The resistances of the auxiliary coils A, B were so chosen that the change of position of S with the greatest magnetising field used amounted to about 16 cms. The difference of the fractional increments of resistance of P and Q $\left(\frac{\Delta P}{P}-\frac{\Delta Q}{Q}=\Delta\phi\right)$ was calculated from the observed displacement of S from its zero position, and the results given below represent values of $\delta\phi$ for different field-strengths.

Measurement of Magnetisation.

The magnetisation of the iron was determined in separate experiments. For this purpose a narrow glass tube, 60 cm. long, was filled with a number of lengths of iron wire, cut from the same specimen, from which the insulation had been removed. A number of turns of fine insulated copper wire was wound on the glass tube near its middle and connected to a ballistic galvanometer, standardised by solenoid and secondary coil. The tube, with the lengths of iron wire, was placed within the magnetising coil, and the BH curve of ascending reversals was determined for the iron in the usual way, and the magnetisation I calculated from the equation $I = B - H/4\pi$. No account was taken of the demagnetising factor of the iron wires; the factor for a cylinder of the same length and of cross-section equal to the sum of the section of the thirteen pieces of wire in the glass tube is but of the order 0·0008.

Results.

The following are particulars as to the coils, &c., used :—

Diameter of soft iron wire used in resistance and *magnetisation experiments*	0·0745 cm.
Resistance of iron coil P (longitudinally wound)	1·045 ohms.
Resistance of iron coil Q (spirally wound)	1·118 „
Do., do., auxiliary coil A	1·382 „
Do, do., auxiliary coil B	1·476 „

The coils A, B were of German silver. Resistance per centimetre of bridge wire (σ) = 0·0000536 ohm. The difference of the fractional increments of resistance of P and Q was calculated from the approximate equation

$$\Delta\phi = \frac{\Delta P}{P} - \frac{\Delta Q}{Q} = \frac{A+B}{AB}\sigma\delta x,$$

where δx is the displacement of the contact S (fig. 1) from its zero position, *i.e.*, from its position when the wires were not magnetised.

The mean temperature of the iron wire during both the change of resistance and the magnetisation measurements, determined by frequently observing the temperature of the water entering and leaving the magnetising coil, was 5·5° C.

The results of all the measurements are shown in the accompanying curves. Fig. 2 is the magnetisation curve (I, H, c.g.s.) of ascending reversals. Fig. 3 the curve B, H for the longitudinal coil. Fig. 4 shows $\Delta\phi$ as a function of the magnetising field H. The general resemblance between this curve and the magnetisation curve suggests that the change of resistance depends on the magnetisation rather than on magnetising force.

Fig 3

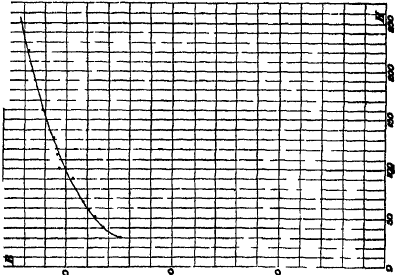

B
15,000
10,000
5000
0
50
100
150
200
250
H

Fig 2

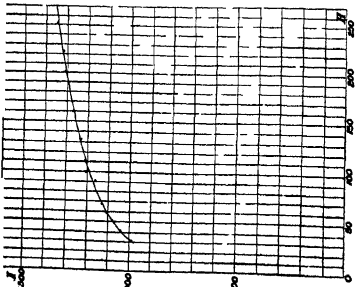
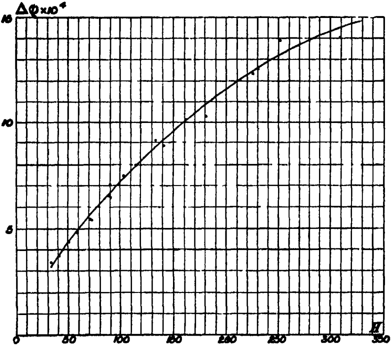

I
4500
4000
500
0
50
100
150
200
250
H

FIG 4



Figs 5 6 7 show $\Delta\phi$ as a function of B I^4 I respectively Of these fig 6 approximates closely to a straight line except in the neighbourhood of the origin It may thus be stated as an empirical result for the specimen of soft iron and for the range of magnetisation employed in these experiments that the change of resistance is approximately proportional to the fourth power of the magnetisation

Note added June 18—The results are complicated to some extent by hysteresis It was found that when the iron wires were first thoroughly demagnetised and after having been left long enough to take the temperature of the surroundings were subjected to a rather strong magnetising current kept in for less than a second so that no appreciable heating could arise about a third of $\delta\phi$ remained after the magnetic force was removed

The above experiments were carried out in the Physical Laboratory of the University College of North Wales and we wish to take this opportunity of expressing our high appreciation of the value of the assistance of two students of the College Mr Guy Barlow and Mr Godfrey Rotter, who, by making many measurements and calculations, enabled us to complete the work

FIG 5

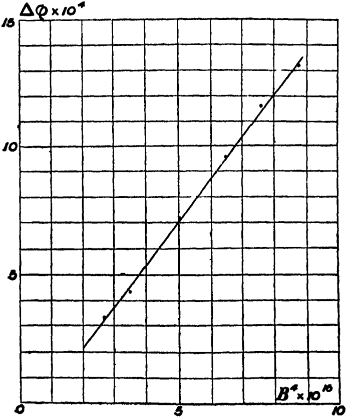


FIG 6

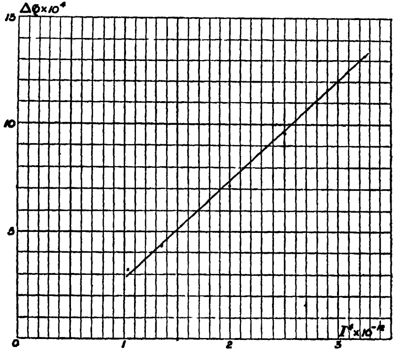
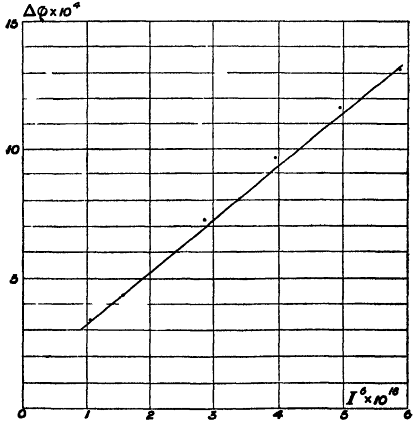


Fig. 7



"The Exact Histological Localisation of the Visual Area of the Human Cerebral Cortex." By JOSEPH SHAW BOLTON, B.Sc., M.D., B.S. (Lond.). Communicated by Dr. MOTT, F.R.S.

Received May 11,—Read June 14, 1900.

(Abstract.)

Previous Research.

The previous research concerning the human visual area has been carried out in three directions.

(1) The study of lesions causing blindness.

(2) The study of the myelination of the corona radiata.

(3) The histological examination of "occipital" or "calcarine" cortex as regards—

(*a*) Cell form.

(*b*) Subdivision of this variety of cortex into layers.

(*c*) The modifications caused in (*a*) and (*b*) by long-standing blindness.

Examination of the literature on the first two subdivisions demonstrates the extreme diversity of opinion which exists regarding the situation of the primary visual area of the cortex.

The object of the present research has been to indicate the exact region of the cortex to which the visuo-sensory function is limited. For this purpose it has been unnecessary to pay attention to the special neuronic structure of this portion of the cerebrum, but the general histology of the cortex referred to in (3), (*b*), and (*c*) has been considered minutely in the third section of this paper.

The Exact Distribution of the "Occipital" Lamination.

(1) The "occipital" lamination in the region of the calcarine fissure has been histologically mapped out, in six normal and pathological brains, as a well defined cortical area.

(2) The general distribution of this area is as follows. It occupies—

(*a*) The body of the calcarine fissure, including the anterior and posterior annectants, and extending upwards to the parallel cuneal sulcus and downwards to the collateral fissure.

(*b*) The posterior part of the calcarine fissure extending to the polar sulci surrounding its extremities.

(*c*) The inferior lip of the stem of the calcarine fissure (including the superficial surface and lower lip of the cuneal annectant) nearly to its anterior extremity, just posterior to which the area tails off to a sharp point.

(3) The approximate outline of this area is consequently pear shaped with the apex anteriorly and the thick end at the pole of the hemisphere.

(4) The area is much decreased in extent, but not in distribution, in cases of old-standing optic atrophy.

(5) In anophthalmos the area is much contracted as regards both extent and distribution. It occupies the usual position in the stem of the calcarine fissure, but only extends backwards as far as the posterior cuneo-lingual annectant, and it is confined to a portion of the inferior lip of the fissure and to the cortex between this and the collateral sulcus.

The General Histology of the Cortex Cerebri in the Region of the Calcarine Fissure.

(1) The following classification of layers has been adopted for the purposes of micrometer measurements :—

(*a*) The cortex of the area of special lamination which has just been described.

- I. The superficial layer of nerve fibres
- II. The layer of small pyramidal cells.
- III*a*. The outer granule layer.
- III*b*. The middle layer of nerve fibres, or line of Gennari.
- III*c*. The inner granule layer.
- IV. The inner layer of nerve fibres.
- V. The layer of polymorphic cells.

(*b*) The cortex surrounding the area of special lamination.

- I. The superficial layer of nerve fibres.
- II. The layer of small and large pyramids.
- III. The layer of granules.
- IV. The inner layer of nerve fibres.
- V. The layer of polymorphic cells.

At the junction of these two varieties of lamination an abrupt change takes place, the line of Gennari suddenly ceasing, and the outer granule layer joining the inner one, the conjoined layer being approximately of the thickness of the former outer layer.

(2) The average of very numerous micrometer measurements of the cortex of the area of special lamination and of the neighbouring convolutions gives the following results :—

(*a*) In the area referred to, in cases of old-standing optic atrophy, the line of Gennari is decreased nearly 50 per cent. in thickness, and the outer granule layer more than 10 per cent.

(*b*) On the other hand, in the cortex surrounding the area referred to, old-standing optic atrophy causes no modification of the lamination.

(*c*) In anophthalmos the conjoined outer granule layer and line of Gennari (for the granules in the former layer are not sufficiently obvious to admit of easy micrometer measurement alone) are narrowed down to two-thirds of the normal thickness, the other layers of the cortex being approximately unchanged. This amount of narrowing is the same as that found in cases of old-standing optic atrophy.

(*d*) The majority of the layers of the cortex either inside or outside the area of special lamination do not vary appreciably in thickness as a result of age or chronic insanity, but there is an almost exact correspondence between the thickness of the conjoined first and second layers of the cortex and the degree of amentia or dementia existing in the patient.

Summary of Conclusions drawn from the present Research.

(1) The area located and described in this paper is the primary visual region of the cortex cerebri

(2) The part of this area to which afferent visual impressions primarily pass is the region of the line of Gennari

(3) A marked contraction of the area in both extent and distribution, without absence of the line of Gennari, occurs in anophthalmos.

(4) This area can probably be described as the cortical projection of the corresponding halves of both retinæ. In this projection the part above the calcarine fissure represents the upper corresponding quadrants and the part below the lower corresponding quadrants of both retinæ.

"Underground Temperature at Oxford in the Year 1899, as determined by Five Platinum Resistance Thermometers."

By Arthur A. Rambaut, M.A., D.Sc., Radcliffe Observer. Communicated by E. H. Griffiths, F.R.S. Received May 17,—Read June 21, 1900.

(Abstract.)

I. *Description of the Apparatus.*

The instruments with which the earth-temperatures given in this paper were observed were five platinum resistance thermometers of the Callendar and Griffiths pattern.

The thermometers were inserted in undisturbed gravel, the first four lying one under the other in a vertical plane beneath the grass of the south lawn of the Radcliffe Observatory, and within a few feet of the Stevenson's screen in which the dry bulb and the wet bulb, the maximum and minimum thermometers, are suspended. A fifth thermometer was subsequently placed at a depth of about 10 feet in a separate pit. The actual depths of the various thermometers as measured in October, 1898, were as follows —

Thermometer.	I.	II.	III	IV.	V.
Depth ...	6½ in.	1 ft. 6 in.	3 ft. 6½ in	5 ft. 8½ in.	9 ft 11½ in.

The resistance box is in its general design similar to that described by Mr. Griffiths,* but simplified to suit the particular class of work for which it was intended It is provided with three principal coils, A, B, and C, whose nominal values are, 20, 10, and 80 box units respectively, a box unit being about 0 01 ohm.

The apparatus is provided with a slow motion contact maker, of Mr. Horace Darwin's pattern,† and Mr. Griffiths's thermo-electric key.‡

In the standardisation of the apparatus the method described by Mr. Griffiths, in his article in 'Nature,' referred to above, was in the main followed. The temperature coefficient was determined by Mr. Griffiths, in his own laboratory at Cambridge. Two separate series of observations led to the following results :—

Date.	Range of temperature	Temperature coefficient.
July 27	9·18°	0 000242
August 8............	12 51	0·000240

The value actually used in the reductions was 0·00024.

From observations made when the instrument was mounted *in situ* at Oxford, the values of the coils were found to be

$$\left.\begin{aligned} C &= 80{\cdot}158 \\ B &= 39{\cdot}979 \\ A &= 19{\cdot}863 \end{aligned}\right\}\text{mean box units,}$$

and one scale division of the bridge wire is equal to

1·0134 mean box units.

One of the most important considerations in connection with this subject is the degree of permanence in the fundamental points, as determined at considerable intervals of time; but the process of standardisation is not one that can be very frequently applied.

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* 'Nature,' vol. 53, November 14, 1895.

† 'Nature,' vol. 53, November 14, 1895.

‡ 'Phil. Trans.,' A, pp. 397–8, vol. 184 (1893).

All the instruments were very carefully standardised by means of observations extending over three days, .in October, 1898, and on October 6, 1899, taking advantage of a visit from Mr. Griffiths, I had the 6-in. thermometer dug up, and we examined its zero point after exactly a year's continuous observations. The readings agreed to within 0·004° C., being

In 1898	0 306
In 1899	0·302

For reasons given in the paper, it was not thought necessary to re-examine the boiling point. For another thermometer (A), kept in the observing room, the fundamental interval was found to have remained practically unchanged, being

In 1898	101 067
And in 1899	101·059

II. *Discussion of the Observations.*

The first step in the discussion of the observations is to group them into monthly means, and thence to deduce the harmonic expressions which will represent the readings of each thermometer throughout the year.* These monthly means expressed in degrees Fahrenheit are given in the following table :—

Mean Monthly Temperature of the Ground at the Radcliffe Observatory, Oxford, 1899.

Thermometer......	1	2	3	4	5
Depth............	6¼ in.	1 ft. 6 in.	3 ft. 6¼ in.	5 ft. 8¼ in.	9 ft. 11¼ in.
January	40°·47	42°·07	44°·68	46°·80	49°·97
February	40·09	41·34	43·25	45·08	48·38
March............	41·84	41·91	43·21	44·74	47·42
April.............	48 77	47·66	46·61	46·40	47·37
May..............	54·86	52·95	50·96	49·54	48·53
June	66·73	62·89	58·29	54·73	50·88
July..............	69·48	65·98	62·15	58·74	53·85
August	69·28	67·79	64·88	61·66	56·39
September	59·34	61·12	62·08	61·19	57·80
October...........	48·99	51·14	54·29	56·12	56·71
November.........	46·73	48·43	50·99	52·69	54·48
December	38·14	41·08	46·35	48·58	52·33

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* Professor W. Thomson, "On the Reduction of Observations of Underground Temperature," 'Trans. Roy. Soc. Edin.,' vol. 22, p. 409.

The harmonic expression to represent the temperature for any thermometer will be

$$\theta = a_0 + a_1 \cos \lambda t + a_2 \cos 2\lambda t + \&c$$
$$+ b_1 \sin \lambda t + b_2 \sin 2\lambda t + \&c \qquad \text{(C)},$$

or

$$\theta = a_0 + P_1 \sin (\lambda t + E_1) + P_2 \sin (2\lambda t + E_2) + \qquad \text{(D)}$$

where t denotes the time represented as the fraction of the year and λ is equal to 2π From the monthly means given above we deduce the following —

Values of the Coefficients

N	a_0					I	E	1_2	Γ_2
5	5 005	1 970	4 706	0 068	+0 37	5 10	4 8	0° 541	35 45 1
4	52 189	6 379	5 418	+0 661	+1 0[illegible]9	8 30	230 10 9	1 [illegible]23	32 42 1
3	5 [illegible]4	−9 46	4 7 5	+1 370	+1 1 3	10 581	243 28 6	1 771	50 39 5
	2 018	1[illegible] 082	3 128	[illegible] 190	+0 76	13 306	25 1 3	2 343	65 23 3
1	5 010	−15 337	1 507	+3 017	+0 511	15 411	64 3	3 060	60 28·2
Air	50 3 6	12 770	1 6 1	+2 847	+1 410	1 878	62 46 1	3 1 7	63 39

From each wave as observed at any pair of thermometers we obtain two determinations of the diffusivity (k) of the gravel one from the diminution of amplitude and the other from the retardation of phase

In computing the value of the expression $\sqrt{(\pi/k)}$ the Paris foot and the Fahrenheit degree have been used

I have omitted the results for the thermometer No 1 (6 inches), which are too much affected by the diurnal changes and other causes From six comparisons of the amplitude and retardation of the annual wave at the remaining four thermometers we obtain twelve determinations of the value of $\sqrt{(\pi/k)}$, the mean of which is 0 1189 For the half yearly wave the mean value obtained in a similar way is 0 1187 This close agreement of the mean values of $\sqrt{(\pi/k)}$ derived from the annual and half yearly waves is very remarkable, and seems to indicate a high degree of precision in the results

The paper deals with the observations of a single year, and the results accordingly exhibit some discrepancies between theory and observations which, although they are less than might have been expected, are greater than one would like to see These discrepancies are due partly to the fact that the temperature variations are not strictly of a periodic character, as the theory supposes, and as such they might be expected to be diminished in the mean of a number of years and partly to irregularities physical and formal, in the surface of the ground

Another source of irregularity affecting previous observations of this sort namely thermometer errors arising from the uncertainty as to the temperature of the liquid in the long stems of the mercury or alcohol thermometers does not in this case apply and if other errors peculiar to the platinum thermometers exist they seem to be confined within much smaller limits

On the Kinetic Accumulation of Stress illustrated by the Theory of Impulsive Torsion

By Karl Pearson F R S Professor of Applied Mechanics University College London

Received May 29—Read June 21 1900

(Abstract)

1 It is usual in engineering practice to double the value of the stresses calculated statically when a live load comes onto a girder and further various empirical laws such as those due to Wohler are adopted in the case of repeated loading to measure the effective resistance of a structure While these methods practically adopted show very clearly that there is a just appreciation that loading varying with the time differs in its nature very considerably from purely permanent loading, they yet fall considerably short of the definiteness required from the theoretical standpoint Occasionally it must be confessed that they would fail even from the practical standpoint were it not for the large factor of safety usually adopted

So soon as a live load comes onto a girder even without impulse, vibrational terms arise in the strains and the same thing occurs also in the parts of machinery subjected to external forces changing with the time The discussion of the strains in a girder due to a rolling load was first undertaken by Sir George Stokes in 1849,[*] and his results have been considerably extended in later papers by Phillips, Renaudot, Bresse and de Saint-Venant[†] The latter has further dealt with a considerable number of problems of what I have elsewhere termed *non impulsive resilience*[‡] as well as a variety of cases of impulsive resilience in the case of bars receiving longitudinal or transverse impacts[§] The numerical results of Saint-Venant's papers, as well as his graphical representations, hardly seem

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[*] See 'History of Elasticity' vol 1 arts. 1276 and 1417

[†] *Loc cit*, vol 2 arts 372—382

[‡] *Loc cit* vol 2 arts 355—357

[§] *Loc cit* vol 2 arts 401—414 (For the history of the subject see art 341) to have been sufficiently considered in the light of practice. They show that there can be a kinetic accumulation of stress at sections where a wave of vibrations is reflected, and that in many cases this kinetic stress can considerably exceed double the value of the statical stress at that section due to the same load permanently applied. In fact, every case of repeated loading or live-loading forms a kinetic problem which must be independently solved, and which, if it presents difficulties, still presents difficulties such as the mathematician is in duty bound to overcome.

While the papers to which I have referred give a fairly complete solution of a number of problems in longitudinal and transverse loading of bars and girders, the problem of torsional loading seems to have been untouched up to the present. Yet the question of torsional vibrations is one that arises very frequently, as in the cases of shafting and axles. The present memoir endeavours to give from the mathematical standpoint a fairly comprehensive solution of the problem of the kinetic accumulation of stress in bars, shafts, axles, &c., owing to repeated, impulsive, or changing systems of torsional loading. The attempt—following the traditions of de Saint-Venant—has been made, however, not to leave the results in the form of long series unintelligible to the average practical man. A very large amount of numerical reduction has been undertaken, and many of the results are shown graphically. The whole of the calculations, as well as the original large diagrams, are my own work; but I owe to Mr. G. Baker and Mr. J. Longbottom, who have at one time or another been my assistants at University College, the reduced diagrams which accompany this paper, and I have to cordially thank them for the care and labour they have given to the reproduction of my drawings.

2. With regard to the general conclusions of the memoir, I should wish to draw special attention to the following points:—

(*a*.) When a load is repeated, or applied, reversed, and repeated, there will *in general* be kinetic accumulations of stress; the amount of this accumulation varies with the times of incidence and of release of the load, but it may easily exceed the double of the stress due to the statical application of the load.

(*b*.) Wöhler's empirical laws must either be considered as allowing for this kinetic accumulation, or not. If they do not allow for it, but are based merely on the assumption of a statical load gradually applied and gradually removed, then they do not cover an immense range of repeated loading which occurs in practice. If they do allow for it, then the conception of the material being worn out by a maximum stress lower than the elastic limit is a false one, for the explanation of the destruction of the material lies in the kinetic accumulation of a much greater stress.

(*c.*) The solution of the problem in both the cases of long shafts and short axles depends upon the discovery of a series of discontinuous functions. These functions would appear to have considerable interest for the mathematician. Like functions first appear, I think, in a paper by Boussinesq,* and they offer an alternative to the usual solution of problems in vibration by Fourier's series. The latter in such cases often give easy analytical expressions, which, however, may be almost useless for the purposes of numerical calculation owing to the slowness of their convergence. In the course of the paper the numerical solution by the use of discontinuous functions, is compared for one case with the solution obtained by a Fourier's series. A verification of the work is thus obtained, and the advantages of the novel functions illustrated.

(*d.*) There are many points in the memoir which suggest possibilities for physical research, and it seems to me that both from the purely scientific and the engineering sides a well-devised series of experiments on continuously and on abruptly varying torsional loads would lead to results of much interest and practical value.

The memoir endeavours to complete as fully for torsional loading the theory of a changing load as the latter has been completed for longitudinal and transverse loading by de Saint-Venant, Boussinesq, and Flammant. The methods, analytical and graphical, are analogous, but the whole of the results, algebraic and numerical, are, I believe, novel, and apply to a series of cases which, if possible, have even greater practical importance.

"The Nature and Origin of the Poison of *Lotus Arabicus*. Preliminary Notice." By WYNDHAM R. DUNSTAN, M.A., F.R.S., Sec.C.S., Director of the Scientific Department of the Imperial Institute, and T. A. HENRY, B.Sc. Lond., 'Salters' Company's Research Fellow. Received June 7—Read June 14, 1900.

Lotus Arabicus is a small leguminous plant resembling a vetch, with pink flowers, indigenous to Egypt and Northern Africa. It grows abundantly in Nubia and is especially noticeable in the bed of the Nile from Luxor to Wady Halfa. It is known to the natives as "Khuther," and old plants with ripe seed are used as fodder. The dried plant is unusually green, and possesses the aroma of new-mown hay. At certain stages of its growth it is highly poisonous to horses sheep, and goats the poisonous property being most marked in the young plant up to the period of seeding Owing to the trouble which this plant has given to the military and civil authorities in Egypt the assistance of the Director of Kew was sought in order that the precise nature of the poison might be ascertained and if possible a remedy found The matter having been referred to the Scientific Department of the Imperial Institute Mr E A Floyer Director of Egyptian Telegraphs collected some of the material for investigation

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* See 'History of Elasticity' vol. 2, arts. 401, 402.

It was found that when moistened with water and crushed the leaves of the plant evolved prussic acid in considerable quantity the amount being greatest in the plant just before and least just after the flowering period Further investigation has shown that the prussic acid originates with a yellow crystalline glucoside ($C_{28}H_{31}NO_{16}$), which it is proposed to name *lotusin* Under the influence of an enzyme, also contained in the plant lotusin is rapidly hydrolysed forming *prussic acid*, *sugar*, and *lotoflavin* a new yellow colouring matter

The hydrolysis may be effected by dilute acids but is only very slowly brought about by emulsin and not at all by diastase The peculiar enzyme which it is proposed to call *lotase* appears to be distinct from the enzymes already known Its activity is rapidly abolished by contact with alcohol and it has only a feeble action on amygdalin Old plants are found to contain lotase but no lotusin

The *sugar* has been proved to be identical with ordinary dextrose

Lotoflavin, the yellow colouring matter has the composition expressed by the formula $C_{15}H_{10}O_6$ It belongs to the class of phenylated pheno-γ pyrones, and is a dihydroxychrysin, isomeric with luteolin, the yellow colouring matter of *Reseda luteola*, and with fisetin, the yellow colouring matter of *Rhus cotinus*

The decomposition which ensues on bringing lotase in contact with lotusin, as happens when the plant is crushed with water, is therefore probably expressed by the following equation —

$$\underset{\text{Lotusin}}{C_{28}H_{31}NO_{16}} + 2H_2O = \underset{\text{Lotoflavin}}{C_{15}H_{10}O_6} + \underset{\text{Prussic acid}}{HCN} + \underset{\text{Dextrose}}{2C_6H_{12}O_6}$$

Hydrocyanic (prussic) acid occurs in small quantity in many plants, and according to Treub and Greshof is often present in the free state The only glucoside at present definitely known which furnishes this acid is the well known amygdalin of bitter almonds, which under the influence of the enzyme emulsin, also contained in the almond, breaks up into dextrose, benzaldehyde, and prussic acid.

Owing to the scientific interest which attaches to this new glucoside, its properties and those of its decomposition products have been very fully studied, and the characteristics of the new enzyme have also been investigated.

We are much indebted to Mr Floyer for the great pains he has taken to collect in Nubia the necessary material for this investigation, and also to Sir W T Thiselton Dyer for having grown the plant at Kew, from seed obtained from Egypt

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On the Spectroscopic Examination of Colour produced by Simultaneous Contrast

By GEORGE J BURCH M A
Reading College Reading

Communicated by FRANCIS GOTCH F R S Professor of Physiology University of Oxford
Received June 12—Read June 21 1900

In a previous communication I have described some methods of using the spectroscope to analyse sensations of successive contrast In those experiments the eye after having been fatigued by monochromatic—preferably spectral—light is exposed to a second stimulus, consisting also of spectral light exciting one or more colour sensations which may or may not include that fatigued by the primary sensation The question naturally arises whether the spectroscopic method might not be applied to problems of simultaneous contrast

With this view I made a number of experiments with the Marlborough spectroscope during the summer of 1897 of which the following may be mentioned A piece of thin cover glass was fixed in front of the eye piece at an angle of 45° with the optic axis so as to reflect into the field of view a small complete spectrum furnished by a 3½ inch direct vision spectroscope In order that this might be visible against the bright field of the larger spectroscope a glass disc with an opaque spot of the required size painted on it was inserted in the eye piece close to the diaphragm

With this arrangement it was easy to see the effect of contrast upon the smaller spectrum but the lack of a comparison spectrum made the experiment far less striking than I had anticipated

Recently a device has occurred to me by which this difficulty may be got over namely, the production of simultaneous contrast by different colours in the two eyes

This method is employed in the well known experiment by Hering, to show that the apparent alteration of colours by contrast is not due to an error of judgment, but to some real effect produced in the eye itself

An ordinary stereoscope is very convenient for this purpose, a square of red glass being inserted on one side of the central partition and a square of blue glass on the other A small black wafer is then fixed at the centre of each glass, with a white wafer close to the left side of the one on the right hand glass, and another on the right side of that on the left-hand glass. The black wafers being the only spots common to both fields are easily fixed binocularly, and the white wafers, each seen with a different eye against a different colour, appear on either side of the combined black spots.

Under these circumstances, although the blue and red fields combine more or less to produce a purple sensation, each white spot retains the contrast colour due to that constituent of the coloured background which alone affects the eye in which its image is formed.

It was only necessary to find some method of substituting for the white spots two small spectra in order to demonstrate the cause of the greenish-blue appearance of the white spot on the red glass, and of the orange hue of the white spot on the blue glass. To do this, I place over each eye-lens one of Thorp's replicas of Rowland's gratings having 15,000 lines to the inch. Two slits are held in a frame in front of the aperture by which light is usually admitted when using the stereoscope for opaque photographs. The spectra of the first order of these slits appear in the middle of the two glasses. In order to prevent direct admixture of the colours of each spectrum with those of the opposite backgrounds, two opaque squares of black material are cemented to each of the coloured glasses, so shaped as to appear of the exact size and position of the spectra. On looking through the stereoscope, two spectra are seen side by side on a field, the colour of which continually oscillates from red through purplish-grey to blue. That connected with the red glass shows little or no red, but a splendid green and an equally splendid violet; while that belonging to the blue glass has the red well developed, the green pale and dingy, and the blue almost absent. The effect of varying the nature of the blue screen is very instructive. With cobalt glass the red is not very bright, owing probably to the transmission of some red rays by the cobalt glass, but the addition of a film stained with Prussian blue, by which these rays are absorbed, greatly improves the red. On the other hand, a pale yellow film which cuts off the violet causes the violet of the spectrum on the blue ground to stand out brightly, while a purple film brings out the green, which, owing to the green light transmitted by ordinary cobalt glass, is generally a good deal enfeebled. In each case the contrast of the two spectra seen by different eyes is so well marked that the experiment seems likely to be of service in teaching. It should not, however, be forgotten that the conditions are not quite so simple as in the ordinary production of artificial colour blindness, and that the results are also somewhat more complex.

Hering's contention, that contrast phenomena originate in the eye rather than in the mind, is substantiated, but the complementary colour to red is shown to consist not of one simple colour-sensation but of two at least, namely, green and violet, and in my own case of blue also. Against a magenta background the complementary colour is seen to be spectral green But in this case the physical stimulus is complex. On adding to the magenta a yellow glass, to cut out the violet, or using candle light, the violet reappears in the complementary spectrum, while if a blue glass is added instead, the violet vanishes, and red stands out brightly in the spectrum. It may be thus shown that the colour which has green for its complementary is not spectroscopically simple, and since the spectral elements of it have each a different and independent effect upon the spectrum of the complementary colour, I conclude that the green sensation has no special connection with the red, or indeed with any single colour sensation

It would, of course, be easy to arrange the apparatus so as to use pure spectral colours for the backgrounds, but the phenomena are sufficiently distinct for ordinary purposes with coloured glasses

A portion of the apparatus used has been paid for out of the sum of £10 allotted to me by the Royal Society from the Government Grant.

"An Experimental Investigation into the Flow of Marble." By FRANK D. ADAMS, M.Sc., Ph D Professor of Geology in McGill University, Montreal, and JOHN T. NICOLSON, D.Sc., M Inst C E., Head of the Engineering Department, Municipal Technical School, Manchester. Communicated by Professor H. L. CALLENDAR, F R S. Received June 12,—Read June 21. 1900.

(Abstract.)

That rocks, under the conditions to which they are subjected in certain parts of the earth's crust, become bent and twisted in the most complicated manner is a fact which was recognised by the earliest geologists, and it needs but a glance at any of the accurate sections of contorted regions of the earth's crust which have been prepared in more recent years to show that there is often a transfer or "flow" of material from one place to another in the folds. The manner in which this contortion, with its concomitant "flowing," has taken place is, however, a matter concerning which there has been much discussion, and a wide divergence of opinion. Some authorities have considered it to be a purely mechanical process, while others have looked upon solution and redeposition as playing a necessary *rôle* in all such movements. The problem is one on which it would appear that much light might be thrown by experimental investigation. If movements can be induced in rocks under known conditions, with the reproduction of the structures found in deformed rocks in nature, much might be learned concerning not only the character of the movements, but also concerning the conditions which are necessary in order that the movements in question may take place.

It is generally agreed that three chief factors contribute to bringing about the conditions to which rocks are subjected in the deeper parts of the earth's crust, where folding with concomitant flowing is most marked. These are —

1. Great pressure.
2. High temperature.
3. Percolating waters.

With regard to the first factor, it must be noted that mere cubic compression does not produce movements of the nature of flowing, although it may produce molecular rearrangement in the rock. A differential pressure is necessary to give movement to the mass. As Heim has pointed out, there is reason to believe that "Umformung ohne Bruch" takes place when a rock is subjected to a pressure which, while greater in some directions than in others, in every direction exceeds the elastic limit of the rock in question. Whether all these factors, or only certain of them, are actually necessary for the production of rock deformation is a question which also requires to be determined by experiment, for by experiment the action of each can be studied separately, as well as in combination with the others.

In the paper of which this is an abstract, a first contribution to such a study is presented, pure Carrara marble being the rock selected for study. The investigation is now being extended to various other limestones, as well as to granites and other rocks.

In order to submit the marble to a differential pressure, under the conditions above outlined, it was sought to enclose the rock in some metal having a higher elastic limit than marble, and at the same time possessing considerable ductility. After a long series of experiments, heavy wrought-iron tubes of special construction were adopted. These were made, following the plan adopted in the construction of ordnance, by rolling thin strips of Low Moor iron around a bar of soft iron, and welding the strips successively to the bar, as they were rolled around it. The core of soft iron composing the bar was then bored out, leaving a tube of Low Moor iron, the sides being about ¼ inch in thickness, and so constructed that the fibres of the iron ran around the tube instead of being parallel to its length. These were found to answer the requirements admirably.

The following procedure was then adopted. Columns of the marble, an inch or in some cases 0·8 inch in diameter and about 1·5 inch in length, were accurately turned and polished. The tube was then very accurately fitted around the marble. This was accomplished by giving a very slight taper to both the column and the interior of the tube, and so arranging it that the marble would only pass half way into the tube when cold. The tube was then expanded by heating, so as to allow the marble to pass completely into it and leave about 1·25 inch of the tube free at either end. On allowing the tube to cool, a perfect contact between the iron and the marble was obtained. In some experiments the tube was subsequently turned down, so as to be somewhat thinner immediately around the marble Into either end of the tube, containing the column, an accurately fitting steel plug or piston was then inserted, and by means of these the pressure was applied The high pressure required was obtained by means of a powerful press, especially constructed for the purpose, consisting of a double hydraulic "intensifier," the water pressure being in the first instance obtained from the city mains By means of this machine, pressures up to 13,000 atmospheres could be exerted on the columns having a diameter of 0·8 inch, and the pressures could be readily regulated and maintained at a constant value for months at a time, if required

It having been ascertained that the columns of the marble 1 inch in diameter and 1½ inch in height crushed at a pressure of from 11,430 to 12,026 lbs. to the square inch, the column enclosed in its wrought-iron tube, in the manner above described, was placed in the machine and the pressure applied gradually, the exterior diameter of the tube being accurately measured at frequent intervals. No effect was noticeable until a pressure upon the marble, varying of course with the thickness of the enclosing tube, but generally about 18,000 lbs. to the square inch, was reached; when the tube was found to slowly bulge, the bulge being symmetrical and confined to that portion of the tube surrounding the marble. The distension was allowed to increase until the tube showed signs of rupture, when the pressure was removed and the experiment concluded. The conditions under which the marble was submitted to pressure were four in number:—

1. At the ordinary temperature in the absence of moisture. (Cold dry crush.)
2. At 300° C. in the absence of moisture. (Hot dry crush.)
3. At 400° C. in the absence of moisture. (Hot dry crush.)
4. At 300° C. in the presence of moisture. (Hot wet crush.)

Eight experiments were made on marble columns at the ordinary temperature, in the absence of moisture, the rate at which the pressure was applied differing in different cases, and the consequent deformation being in some cases very slow and in others more rapid, the time occupied by the experiment being from ten minutes to sixty-four days. The amount of deformation was not in all cases equal, as some of the tubes showed signs of rupture sooner than others. On the completion of the experiment the tube was slit through longitudinally by means of a narrow cutter in a milling machine, along two lines opposite one another. The marble within was found to be still firm and compact, and to hold the respective sides of the tube, now completely severed from one another, so firmly together that it was impossible without mechanical aids to tear them apart. By means of a steel wedge driven in between them, however, they could be separated, but only at the cost of splitting the marble through longitudinally. The half columns of the marble now deformed generally adhere so firmly to the tube that it is necessary to spread the latter in a vice in order to set them free. The deformed marble, while firm and compact, differs in appearance from the original rock in possessing a dead white colour, somewhat like chalk, the glistening cleavage surfaces of the calcite being no longer visible. The difference is well brought out in certain cases owing to the fact that a certain portion of the original marble often remains unaltered and unaffected by the pressure. This when present has the form of two blunt cones of obtuse angle whose bases are the original ends of the columns resting against the faces of the steel plugs, while the apices extend into the mass of the deformed marble and point toward one another. These cones, or rather paraboles of rotation, are developed, as is well known, in all cases when cubes of rock, Portland cement, or cast iron are crushed in a testing machine in the ordinary manner. In the present experiments they seldom form any large portion of the whole mass.

In order to test the strength of the deformed rock, three of the half columns from different experiments, obtained as above described, were selected and tested in compression. The first of these, which had been deformed very slowly, the experiment extending over sixty four days, crushed under a load of 5350 lbs. per square inch; the second, which had been deformed in 1½ hours, crushed under a load of 4000 lbs. per square inch; while the third, which had been quickly deformed, the experiment occupying only 10 minutes, crushed under a load of 2776 lbs. per square inch. As mentioned above, the original marble, in columns of the dimensions possessed by these before deformation, was found to have a crushing weight of between 11,430 and 12,026 lbs. per square inch. These figures show that, making all due allowance for the difference in shape of the specimens tested, the marble after deformation, while in some cases still possessing considerable strength, is much weaker than the original rock. They also tend to show that when the deformation is carried on slowly the resulting rock is stronger than when the deformation is rapid.

Thin sections of the deformed marble, passing vertically through the unaltered cone and the deformed portion of the rock, were readily made, and when examined under the microscope clearly showed the nature of the movement which had taken place. The deformed portion of the rock can be at once distinguished by its turbid appearance, differing in a marked manner from the clear transparent mosaic of the unaltered cone. This turbid appearance is most marked along a series of reticulating lines running through the sections, which when highly magnified are seen to consist of lines or bands of minute calcite granules. They are lines along which shearing has taken place. The calcite individuals along these lines have broken down, and the fragments so produced have moved over and past one another, and remain as a compact mass after the movement ceased. In this granulated material are enclosed great numbers of irregular fragments and shreds of calcite crystals, bent and twisted, which have been carried along in the moving mass of granulated calcite as the shearing progressed. This structure is therefore cataclastic, and is identical with that seen in the felspars of many gneisses.

Between these lines of granulated material the marble shows movements of another sort. Most of the calcite individuals in these positions can be seen to have been squeezed against one another and in many cases a distinct flattening of the grains has resulted, with marked strain shadows, indicating that they have been bent or twisted. They show, moreover, a finely fibrous structure in most cases, which, when highly magnified, is seen to be due to an extremely minute polysynthetic twinning. The chalky aspect of the deformed rock is in fact due chiefly to the destruction by this repeated twinning of the continuity of the cleavage surfaces of the calcite individuals, thus making the reflecting surfaces smaller. By this twinning, the calcite individuals are enabled under the pressure to alter their shape somewhat, while the flattening of the grains is evidently due to movements along the gliding planes of the crystals. In these parts, therefore, the rock presents a continuous mosaic of somewhat flattened grains.

From a study of the thin sections it seems probable that very rapid deformation tends to increase the relative abundance of the granulated material, and in this way to make the rock weaker than when the deformation is slow.

When the marble is heated to 300° C. in a suitably-constructed apparatus and is then subjected to deformation under conditions which otherwise are the same as before, the cataclastic structure is found to be absent and the strength of the deformed marble rises to 10,652 lbs. to the square inch, that is to say, it is nearly as strong as the original rock. The calcite grains, which in the original rock are practically equidimensional, are now distinctly flattened, some of them being three or even four times as long as they are wide. Some grains can be seen to have been bent around others adjacent to them, the twin lamellæ curving with the twisted grain. In others again of these twisted lamellæ, the twinning only extends to a certain distance from the margin, leaving a clear untwinned portion in the centre. The rock consists of a uniform mosaic of deformed calcite individuals.

When the deformation is carried out at 400° C., no trace of cataclastic structure is seen.

An experiment was then made in which the marble was deformed at 300° C., but in the presence of moisture, water being forced through the rock under a pressure of 460 lbs. per square inch during the deformation, which extended over a period of fifty-four days, or nearly two months. Under these conditions the marble yielded in the same manner as when deformed at 300° C., in the absence of moisture, that is, by movements on gliding planes and by twinning, but without cataclastic action. The deformed marble, however, when tested in compression, was found actually to be slightly stronger than a piece of the original marble of the same shape. The structure developed was identical with that of the marble deformed at 300° C. in the absence of water. The presence of water, therefore, did not influence the character of the deformation. It is quite possible, however, that there may have been a deposition, of infinitesimal amount, of calcium carbonate along very minute cracks or fissures, which thus helped to maintain the strength of the rock. No signs of such deposition, however, were visible.

By studying the marble deformed at a temperature of 300° C., or better at 400° C., it will be seen that structures induced in it by the movements, and the nature of the motion, are precisely the same as those observed in metals when they are deformed by impact or by compression. In a recent paper by Messrs. Ewing and Rosenhain, "Experiments in Micro-metallurgy: Effects of Strain," which appeared in these Proceedings, three photographs of the same surface of soft iron, showing the results of progressive deformation under pressure, are shown, which photographs could not be distinguished from those of thin sections of the marble described in the present paper, at corresponding stages of deformation. In both cases the movements are caused by the constituent crystalline individuals sliding upon their gliding planes or by polysynthetic twinning. In both cases the motion is facilitated by the application of heat. The agreement between the two is so close that the term "flow" is just as correctly applied to the movement of the marble in compression under the conditions described, as it is to the movement which takes place in gold when a button of that metal is squeezed flat in a vice, or in iron when a billet is passed between rolls.

In order to ascertain whether the structures exhibited by the deformed marble were those possessed by the limestones and marbles of contorted districts of the earth's crust, a series of forty-two specimens of limestones and marbles from such districts in various parts of the world were selected and carefully studied. Of these, sixteen were found to exhibit the structures seen in the artificially-deformed marble. In these cases the movements had been identical with those developed in the Carrara marble. In six other cases the structures bore certain analogies to those in the deformed rock but were of doubtful origin, while in the remaining twenty the structure was different.

The following is a summary of the results arrived at:—

1. By submitting limestone or marble to differential pressures exceeding the elastic limit of the rock and under the conditions described in this paper, permanent deformation can be produced.

2. This deformation, when carried out at ordinary temperatures, is due in part to a cataclastic structure and in part to twinning and gliding movements in the individual crystals comprising the rock

3. Both of these structures are seen in contorted limestones and marbles in nature.

4. When the deformation is carried out at 300° C, or better at 400° C, the cataclastic structure is not developed, and the whole movement is due to changes in the shape of the component calcite crystals by twinning and gliding.

5. This latter movement is identical with that produced in metals by squeezing or hammering, a movement which in metals, as a general rule, as in marble, is facilitated by increase of temperature.

6. There is therefore a flow of marble just as there is a flow of metals, under suitable conditions of pressure.

7. The movement is also identical with that seen in glacial ice, although in the latter case the movement may not be entirely of this character.

8. In these experiments the presence of water was not observed to exert any influence.

9. It is believed, from the results of other experiments now being carried out but not yet completed, that similar movements can, to a certain extent at least, be induced in granite and other harder crystalline rocks.

"Lines of Induction in a Magnetic Field." By H. S. HELE-SHAW, F.R.S., and A HAY, B.Sc Received June 13,—Read June 21, 1900.

(Abstract.)

When a viscous liquid flows in a thin layer between close parallel walls, the motion takes place along stream-lines identical with those of a perfect liquid. The course of the stream-lines may be rendered evident by injecting into the clear liquid thin bands of coloured liquid.

If the thickness of the liquid layer be varied, then there will be a decrease of resistance to the flow wherever there is an increase of

thickness. As a consequence, there will be a convergence of the stream lines on the area of greater thickness.

When experiments with liquid layers of variable thickness were first tried, a general resemblance was noticed between the stream lines so obtained and the lines of induction due to the presence of a permeable substance in a uniform magnetic field.

The main object of the present paper was to investigate accurately whether complete correspondence between the two cases really existed, and, should correspondence be established, to apply the method to the solution of a number of two dimensional magnetic problems. The investigation thus involved—

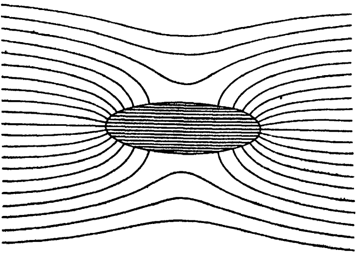
(1) A mathematical treatment of the subject by means of which plotted diagrams could be obtained for comparison with experimental results.

(2) The construction of apparatus capable of giving exact results which could be photographed.

(3) The investigation of the laws connecting the rate of flow with the thickness of film of the liquid used.

The theoretical case selected as a test case was that of an elongated elliptic cylinder placed with its major axis along the field, the permeability being assumed to be 100. The lines of induction for this case are shown in the accompanying diagram and were calculated and plotted by the method explained in the paper.

DIAGRAM



If, for the moment, we assume that the liquid stream lines are identical with lines of magnetic induction, then the following correspondence between the two cases holds —

Liquid Flow.	*Magnetic Induction.*
(*a*) Pressure gradient.	(*α*) Magnetic intensity or force.
(*b*) Rate of flow per unit width of liquid layer.	(*β*) Magnetic induction.
(*c*) Ratio of (*b*) to (*a*).	(*γ*) Permeability = ratio of (*β*) to (*α*).

From this it is evident that the permeability corresponding to a given ratio of thicknesses of the liquid layer is given by the ratio of the rates of flow, per unit width of layer, for the two thicknesses, assuming the same pressure gradient for both. The connection between the rate of flow and the thickness for a given gradient of pressure was carefully investigated in a series of preliminary experiments, and it was found that the rate of flow varied as the *cube* of the thickness—a result which was afterwards confirmed by a theoretical investigation The permeability in the magnetic problem is thus given by the ratio of the *cubes* of the two thicknesses.

A stream-line diagram corresponding to the theoretical diagram given above was next obtained, and on superposing the two it was found that their lines were practically coincident.

The soundness of the method as applied to two-dimensional problems in magnetic induction having been thus established, the authors proceeded to apply it to a number of special cases, many of which could not be successfully attacked by any other method. The paper is accompanied by a large number of photographs, showing the results obtained. Some of these are of importance from an electrical-engineering standpoint.

The method described is the only one hitherto known which enables us to determine the lines of induction in the substance of a solid magnetic body. It is equally applicable to two-dimensional problems in magnetic induction, electrical flow, and heat conduction.

"The Distribution of Molecular Energy." By J. H. JEANS, B.A., Scholar of Trinity College, and Isaac Newton Student in the University of Cambridge. Communicated by Professor J. J. THOMSON, F.R.S. Received June 14,—Read June 21, 1900.

(Abstract.)

This paper attempts to examine the well-known difficulties in connection with the partition of energy in the molecules of a gas. A definite dynamical system is first considered, an ideal gas in which the molecules are loaded spheres, that is, spheres of radius *a*, of which the centre of mass is at a small distance, *r*, from the geometrical centre. It

is shown by direct methods that the energy will, after an infinite time, distribute itself equally between the five degrees of freedom, but when a wave of sound is passed through the gas, the energy will never have sufficient time to attain to its equilibrium distribution. It is shown that sounds of different period will be propagated with appreciably different velocities, except in the extreme case in which the ratio of r to a is almost, but not necessarily quite, zero In this case, the ratio of the two specific heats, as determined from indirect experiments on the velocity of sound, would be $1\frac{2}{3}$, while direct experiments might give any value from $1\frac{2}{3}$ to $1\frac{2}{5}$, the value varying with the duration of the experiment

It is suggested that an escape from this dilemma is made possible by regarding the molecules as forming an incomplete dynamical system, of which the ether is the remaining part. For purposes of illustration, it is imagined that the interaction between the two parts of this complete system consists of a frictional force which retards the rotation of the molecules. A steady state is now impossible, but it is shown that when the energy (*i.e.*, temperature) of the gas is sufficiently low, the gas tends to assume an approximately steady state, in which the energy of rotation vanishes in comparison with that of translation

It is then shown that these conclusions may be generalised, so as to apply to a more complex system of molecules, these molecules possessing an indefinite number of degrees of freedom, and internal potential energy as well as kinetic. The molecules exert forces on one another at any distance, and the radiation is of a more general type than before.

In Part III some of the physical consequences of the view here put forward are examined. The final conclusions are briefly as follows:—

The degrees of freedom must be weighted, not counted. The weight of a degree of freedom may be anything between unity and zero, and may vary with the temperature. A degree of freedom which does not radiate energy will always be of weight unity; for a non-luminous gas, one which does radiate energy when the gas is heated is of weight zero.

As the gas is heated, the radiation and internal energies will increase much more rapidly than the temperature, until finally, at infinite temperature, the energy is distributed equally between all degrees of freedom.

Finally, it is pointed out that this view is in accordance with ordinary thermodynamics for a non-luminous gas, but that the ordinary thermodynamics must be supposed to break down above the temperature of incandescence, a view which has already been put forward, in a modified form, by Wiedemann.

"On the Capacity for Heat of Water between the Freezing and Boiling Points, together with a Determination of the Mechanical Equivalent of Heat in Terms of the International Electrical Units.—Experiments by the Continuous-flow Method of Calorimetry performed in the Macdonald Physical Laboratory of McGill University, Montreal." By HOWARD TURNER BARNES, M.A.Sc., D.Sc., Joule Student. Communicated by Professor H. L. CALLENDAR, F.R.S. Received June 15,—Read June 21, 1900.

(Abstract.)

At the Toronto meeting of the British Association in 1897, a new method of calorimetry was proposed by Professor Callendar and the author for the determination of the specific heat of a liquid in terms of the international electrical units. At the Dover meeting in September, 1899, some of the general results obtained with the method for water over a part of the range between 0° and 100° were communicated, with a general discussion of the bearing of the experiments to the work of other observers. In the present paper the author gives a summary of the complete work, in the case of water, to determine the thermal capacity at different temperatures between the freezing and boiling points.

Theory of the Method.

If a continuous flow of liquid in a tube be made to carry off a continuously supplied quantity of heat EC, in electrical units, then after all temperature conditions have become steady

$$JsQ(\theta_1 - \theta_0)t + (\theta_1 - \theta_0)ht = ECt$$

where

J = mechanical equivalent of heat,
Q = flow of liquid per second,
s = the specific heat of the liquid,
θ_0 = the temperature of the liquid flowing into the tube,
θ_1 = the temperature of the liquid flowing out of the tube,
h = the heat loss per degree rise of temperature from the liquid flowing through,
t = the time of flow.

In the case of water, E represents the E.M.F. across an electrical heating conductor in the tube, and C the current flowing. In this case, which is treated of entirely in the present paper, Js is replaced

by 4.2 (1 ± δ) where δ is a small quantity to be determined and varies with the thermal capacity of the water which is not exactly equal to 4.2 joules at all points of the range.

Substituting in the general equation, rearranging terms and dividing through by t the equation is given in the following form:—

$$4.2Q(\theta_1-\theta_0)\delta+(\theta_1-\theta_0)h = EC-4.2Q(\theta_1-\theta_0)$$

which is termed the general difference equation of the method. The two terms δ and h may be determined by using two values of Q giving two equations of the form

$$4.2Q_1(\theta_1+\theta_0)\delta_1+(\theta_1-\theta_0)h = E_1C_1-4.2Q_1(\theta_1-\theta_0)$$

$$4.2Q_2(\theta_2-\theta_0)\delta_2+(\theta_2-\theta_0)h = E_2C_2-4.2Q_2(\theta_2-\theta_0)$$

For the same value of θ_0, if the electrical supply for the two flows is regulated so that $\theta_1 = \theta_2$, then $\delta_1 = \delta_2 = \delta$, and by eliminating h,

$$\delta = \frac{(E_1C_1-4.2Q_1(\theta_1-\theta_0))-(E_2C_2-4.2Q_2(\theta_1-\theta_0))}{4.2(Q_1-Q_2)(\theta_1-\theta_0)}$$

which corresponds to the mean temperature

$$\theta_0+\frac{\theta_1-\theta_0}{2}$$

where $(\theta_1-\theta_0)$ is not too great.

In the present method the flow tube is of glass about 2 mm. in diameter, connected to two larger tubes forming an inflow and an outflow tube, in which the temperature of the water is read by a differential pair of platinum thermometers before and after being heated by the electric current. A glass vacuum jacket surrounds the fine flow tube and a part of the inflow and outflow tubes to reduce the heat loss as much as possible. A copper water jacket encloses the inflow tubes and vacuum jacket in order to maintain the glass surface of the vacuum jacket always at a constant temperature equal to the inflowing water. The heat loss from the water is then the loss due to radiation from the flow tube through the vacuum jacket, and conduction from the ends of the flow tubes.

In testing the accuracy of the method the dependence of the heat loss on the rise of temperature was found and the dependence of the heat loss on the flow.

Measurement of Fundamental Constants

The electric heating current supplied to the wire conductor in the fine-flow tube was taken from four large 200-ampere hour accumulators. It was passed through a standardised resistance in series with the wire

conductor and in addition a specially constructed rheostat, by which small adjustments to the circuit could be made for regulating the heat supply.

The measurement of the different constants entering into the general difference equation of the method is treated of under two heads, Electrical and Thermal. In the first, the Clark cell and resistance form the principal measurements, and in the second the measurement of temperature, time, and weight have to be considered. An exceedingly accurate potentiometer was employed to determine the difference of potential across the resistance and calorimeter in terms of the E.M.F. of the Clark cell.

Experimental Proof of the Theory of the Method.

In this section the author shows that the dependence of the heat loss per degree rise in the calorimeter varies in a linear relation to the flow in proportion to $4·2\,Q\delta$ beyond certain limits of flow, and that this is essential for the fulfilment of the theory of the method. For very small flows the conduction effect at the outflow end, due to the rise of temperature in the water, appears and causes the line representing the relation of heat loss to flow to approach an infinitely large value of the heat loss for a zero flow. The limits of flow chosen in the present measurements are safely included within the linear relation.

The relation of the heat loss to the rise of temperature shows that for rises of from 2° to 8° and beyond, the heat loss is directly proportional to the rise. The thermal capacity of the calorimeter is calculated, and it is shown that for the small changes in the temperature of the calorimeter during an experiment this is negligible.

Effect of Stream-line Motion.

Some of the earlier results are given in this section, showing the effect of stream-line motion on the distribution of heat throughout the water column for a calorimeter with a 3-mm.-bore flow tube for different flows. The temperature of the heating wire used for these experiments is also calculated, and found to vary considerably when moved from the centre to the sides of the tube. It was found necessary to thoroughly stir the water in its passage through the flow tube, in order to ensure a perfectly uniform temperature throughout the water column.

Preliminary Measurements.

The preliminary measurements of the value of J, which were made in the summer of 1898, were affected by the presence of stream-line motion in the tube, as at that time no device was introduced to obviate it. Owing to the calorimeter which was used then, however, having only a 2 mm.-bore flow tube, the effect was not so large as for the tube with a 3 mm. bore. The value which corresponds to a temperature of the water of 30° C. is 4·1805 joules, which agrees to 1 part in 2000 with the later and more accurate measurements which were obtained for all the calorimeters with the various devices for eliminating the stream-lines.

Experiments between 0° *and* 100°.

In this section the complete list of fifty-five tables is included, giving upwards of forty-five complete experiments at different parts of the range. The experiments have extended over just a year, and divide themselves naturally into eight separate series. The results with different calorimeters and with different rises of temperature are included. Summarising the results and plotting the values of δ for all the experiments, the following values of δ and the corresponding values of J are obtained from the smoothed curve:—

Summary of the Specific Heat of Water from Smoothed Curve.

Temperature. ° C	δ.	J.
5	+0·00250	4·2105
10	−0·00050	4·1979
15	−0·00250	4·1895
20	−0·00385	4·1838
25	−0·00474	4·1801
30	−0·00523	4·1780
35	−0·00545	4·1773
40	−0·00545	4·1773
45	−0·00520	4·1782
50	−0·00480	4·1798
55	−0·00430	4·1819
60	−0·00370	4·1845
65	−0·00310	4·1870
70	−0·00245	4·1898
75	−0·00180	4·1925
80	−0·00114	4·1954
85	−0·00043	4·1982
90	+0·00025	4·2010
95	+0·00090	4·2038
	Mean value	4·18876

The values of δ represent the specific heat of water in terms of a thermal unit equal to 4·2000 joules, which occurs at 9° C. It is more suitable to select a thermal unit at a more convenient part of the scale. The mean value of the mechanical equivalent of heat from these measurements over the whole range is 4·18876 joules, which is very nearly equal to the value at 16° C., which is 4·1883 joules. It seems desirable to select a unit at a temperature which, if at the same time at a convenient part of the scale, may be equal to the mean value over the whole scale. The author has in consequence adopted a unit at 16° C., and has expressed the specific heat of water in the following table in terms of this unit:—

Variation of the Specific Heat of Water in Terms of a Thermal Unit at 16° C.

Temperature ° C.	Observed values	Calculated values
5	1·00530	1·00446
10	1·00230	1·00206
15	1·00030	1·00024
20	0·99895	0·99894
25	0·99806	0·99807
30	0·99759	0·99757
35	0·99735	0·99735
40	0·99735	0·99735
45	0·99760	0·99757
50	0·99800	0·99807
55	0·99850	0·99894
60	0·99910	0·99910
65	0·99970	0·99972
70	1·00035	1·00036
75	1·00100	1·00100
80	1·00166	1·00166
85	1·00237	1·00233
90	1·00305	1·00301
95	1·00370	1·00370
Mean......	1·00012	

In expressing the results in a formula it is impossible to fit any one simple expression over the whole scale. It is seen that the curve falls rapidly from 0°, passes through a minimum point at 37·5°, and increases again less rapidly towards 100°. Two formulæ can be fitted very accurately over the scale. Between 5° and 37·5° C. the following expression in terms of a thermal unit at 16° is found to read,

$$S = 0{\cdot}99733 + 0{\cdot}0000035\,(37{\cdot}5 - t)^2 + 0{\cdot}00000010\,(37{\cdot}5 - t)^3.$$

The same formula holds between 37·5° and 55° by simply considering all values of the cubical term positive. Above 55° the simple formula

$$S = 0{\cdot}99850 + 0{\cdot}000120\,(t - 55^\circ) + 0{\cdot}00000025\,(t - 55)^2$$

holds with great accuracy.

Both these formulæ are given for comparison in the second table. They fit very closely except below 5°, where the specific heat curve increases more rapidly. These lower values are within 1 part in 1000, however.

A summary of the values obtained for the radiation loss shows that the absolute value for any one calorimeter cannot be relied on to an order of accuracy greater than 1 part in 1000 over extended periods. This is particularly true when the temperature of the calorimeter is widely changed. It was found most essential to always eliminate the heat loss from at least two different flows in order to be completely independent of its absolute value. The complete independence of the results from the value of the heat loss, provided this remained constant throughout the time of an experiment, was shown by employing calorimeters with different degrees of vacuum involving widely differing values of the heat loss. The temperature coefficient of the radiation loss was found to be almost exactly linear over the range of these experiments.

Relation to the Work of other Observers.

It is at once apparent that the value of the mean mechanical equivalent of heat obtained from these measurements, which is 4·18876 joules, is somewhat larger than the exceedingly accurate and trustworthy measurements of Reynolds and Moorby. Their value, which is 4·18320 joules, is lower by 0·132 per cent., or a little over 1 part in 1000.

It is evident that this error may be attributed to the neglecting of some correction factor in the present series of experiments at the extremities of the range, which would cause the *variation* curve to increase more rapidly than it truly does; but from the order of accuracy with which the theory of the present experiments holds at the extremities of the range, it is far more likely that the variation curve is correct, and that the difference in the two results is to be attributed to an error in one of the constants. The thermal constants employed in the two different experiments are referred to the same values, but the introduction of the value of the electrical units into the present series of experiments, which do not enter into the calculation of Reynolds's and Moorby's result, renders it highly probable that the error is to be looked for here. In view of the immense amount of labour expended in establishing the value of the international ohm, it is probable that the error is not there. The recent work on the absolute value of the Clark cell, which is demanding so much attention just now, and which has so far given so many inconsistent results, makes it very probable that the value of the Clark cell adopted in the present work is in error. If this is so, then, as is pointed out, the value of the Clark cell must be taken as 1·43325 int. volts at 15° C., in order to bring the present series of experiments, involving both the international volt and ohm, into absolute accord with the result by the direct mechanical method of Reynolds and Moorby.

Having considered the above relationship, the mean value of the mechanical equivalent given by Rowland's experiments between 6° and 36° C. is compared with the same mean value from the present series of experiments over the same range. By expressing this latter value in terms of the value of the Clark cell 1·43325 volts, or as may be said in terms of Reynolds's and Moorby's determination, instead of the original value 1·43420 volts used in calculation, it comes equal to 4·1817 joules. The value obtained from Rowland's corrected curve is 4·1834 joules, which agrees with the present series of experiments to 1 part in 2000. This is a discrepancy so small as to be, if not within the limits of error of these several determinations, at least negligibly small in comparison to the great range covered by the present series of experiments.

By far the most difficult part of the present series of experiments is the comparison of the absolute value of the mechanical equivalent of heat obtained from these experiments with the values obtained by the electrical method used by Griffiths, and by Schuster and Gannon, even when our several results are expressed in terms of the same values of the units used. There is every reason to believe that the values of the resistance standards used in the present work were the same as those used by both these investigators. It is also highly probable that the values of the Clark cells in the present series of experiments were in correct agreement with all the best results that have been obtained in setting up this electro-chemical combination. It is probable that the difference in the values obtained by Griffiths, and by Schuster and Gannon, from the value obtained in the present series of experiments must be attributed not to these, but to the radical difference in the methods of calorimetry.

"Energy of Röntgen and Becquerel Rays and the Energy required to produce an Ion in Gases."

By E. RUTHERFORD, M A., B.Sc., Macdonald Professor of Physics, and R. K. McCLUNG, B.A., Demonstrator in Physics, McGill University, Montreal. Communicated by Professor J. J. THOMSON, F R.S. Received June 15,—Read June 21, 1900.

(Abstract.)

The primary object of the investigations described in the paper was the determination of the energy required to produce a gaseous ion when X rays pass through a gas, and to deduce from the result the amount of energy radiated out into the gas by uranium, thorium, and the other radio-active substances.

In order to determine this "ionic energy" it has been necessary to accurately measure the heating effect of X rays and the absorption of Röntgen radiation in passing through a gas.

The coefficient of transformation of a fluorescent screen excited by X rays as a source of light has also been investigated, and a simple practical method of expressing the intensity of Röntgen radiation in absolute measure has been explained.

The method adopted to determine the ionic energy was briefly as follows:—

The maximum current between two electrodes produced by the ionization of a known volume of the gas by the rays was determined.

In order to ionize the gas energy has to be absorbed, and the intensity of the radiation falls off more rapidly than the law of inverse squares. Assuming that the energy of the radiation absorbed in the gas is expended in the production of ions, then, knowing the coefficient of absorption of the rays in the gas, the total current produced by the complete absorption of the whole radiation given out by the bulb into the gas can be deduced.

Let i = maximum current produced by the total ionization of the gas by the rays,
n = number of ions produced,
ϵ = charge on an ion.
Then $i = n\epsilon$.
Let H = heating effect due to the rays when absorbed in a metal,
E = total energy of the rays in ergs,
Then E = JH, where J = Joule's equivalent.

If W = average energy required to produce an ion, then

$$nW = E = J.H,$$

$$\therefore W = \frac{JH}{n} = \frac{JH\epsilon}{i}.$$

The values of H and i are experimentally determined, and, assuming the value of ϵ, namely, $6{\cdot}5 \times 10^{-10}$ electrostatic unit, determined by J. J. Thomson, the value of W is found in absolute measure.

In the course of the investigation the following subjects have been considered:—

(1) Measurement of the heating effect of X rays.

(2) Efficiency of a fluorescent screen excited by X rays as a source of light.

(3) Absorption of X rays in gases at different pressures.

(4) Determination of the energy required to produce an ion in air and other gases, including deductions on—

(*a*) Distance between the charges of ions in a molecule.

(*b*) Minimum potential difference required to produce a spark.

(5) Energy of Becquerel rays and emission of energy by radio-active substances.

Heating Effect of X Rays.

An automatic focus tube was employed, excited by a large induction coil with a special form of Wehnelt interrupter giving fifty-seven breaks per second. The bulb gave out intense rays of a very penetrating character.

The heating effect was measured by determining the variation of resistance of a special platinum bolometer when the rays fell upon it. A platinum strip, about 3 metres in length, 0·5 cm. wide, and 0·003 cm. thick, was wound on a light mica frame 10 cm. square. Two such "grids," as similar as possible, were constructed, and formed the two arms of a Wheatstone bridge. A balance was obtained for a momentary pressing of the battery key, using a sensitive galvanometer. The rays were then turned on for 30 or 45 seconds, and the deflection from zero determined immediately after the rays were stopped.

In order to measure the heating effect, a current was sent for the same time as the rays acted through the grid, and its value adjusted until the deflection due to the heating of the grid was the same as for the rays. When this is the case the heat supplied per second to the grid by the rays is equal to the heat supplied per second by the current.

Thus, heating effect of rays per second $= 0{\cdot}24 i^2$ R calorie, where

$i =$ current through the grid of resistance R.

The grids were enclosed in a lead vessel with an aluminium window to let in the rays. The whole was surrounded by a felt covering, and several aluminium plates intervened between the bulb and the grid, so that any heating effect, except that due to the rays, was completely eliminated.

About 0·55 of the energy of the incident rays was absorbed in the grid. Some of the energy of the rays was used up in exciting secondary radiation at the surface of the platinum grid, but the amount was not large, and was neglected in comparison with the total energy of the rays.

The rate of supply of heat to the grid area 92·2 sq. cm. at a distance of 26 cm. from the source of rays was

0 00014 grammo-calorie per second

The total energy of the rays given out from the front surface of the platinum antikathode (omitting absorption of rays in the glass of bulb, in air and screens) was

0 011 grammo-calorie per second,

or 0·046 watt.

The number of discharges per second was 57, and assuming 10^{-5} second* as the average duration of the rays during each discharge, the maximum rate of emission of energy from the bulb

= 19 5 calories per second.

The heating effect of the sun's rays falling normally on 1 sq. cm. of surface = 0·035 calorie per second. The maximum rate of emission of energy from an X-ray bulb is thus 560 times greater than the energy of the sun's rays at the surface of the earth.

Some experiments were made on the heating effect of the rays, using a thermopile, but it was found to be a very unsuitable instrument for such a determination.

Efficiency of a Fluorescent Screen.

Photometric comparisons were made of the light from a fluorescent screen with that of the standard Hefner-Alteneck amyl lamp, using a Lummer-Brodhun prism. With a screen of platinocyanide of barium

$$\frac{\text{Intensity of light from screen}}{\text{Intensity of light from amyl lamp}} = 0{\cdot}0206.$$

Tumlirz† has shown that the energy of the visible light from an amyl lamp falling normally on 1 sq. cm. surface at unit distance

= 0·00361 gramme-calorie per second.

For X rays of the same intensity as were used in the photometric measurements, the energy under the same conditions

= 0·0028 calorie,

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* Trouton, 'Brit. Assoc Report,' 1896.

† 'Wied. Annal.,' vol. 38, p. 640.

or the rate of emission of energy per second as visible light from the Hefner lamp is nearly twice the rate of emission of energy from the X-ray tube. 0·73 of the energy of the rays was absorbed in the screen.

The efficiency of the transformation of X rays into visible light by the screen (compared with the Hefner lamp)

$$= 0·044 \quad \text{or} \quad 4·4 \text{ per cent.}$$

Assuming this transformation factor for a fluorescent screen, two simple photometric measurements are required to express the energy of any bulb in absolute measure. The light from a fluorescent screen is first compared with the standard Hefner lamp. The absorption of the rays in the screen is determined by placing a piece of the screen in the path of the rays.

Let ρ = ratio of intensities of light from bulb and lamp,
ρ_1 = ratio of transmitted to incident radiation on the screen.

Then it is shown that the intensity in absolute measure

$$= \frac{0·082\rho}{1-\rho_1} \text{ gramme-calorie per second.}$$

The absorption in the cardboard of the screen is supposed to be negligible, but if necessary can be readily allowed for.

Absorption of X Rays in Gases.

A null method was employed, as the absorption of the rays in air at atmospheric pressure was small. The rays passed through two long brass tubes with aluminium ends, and the current produced by the rays, after passing through one tube, was balanced against the current due to the other. On exhausting one tube the electrometer balance was disturbed. From measurements of the deflection per second from the balance and the deflection per second due to the rays after passing through one tube, the absorption can be calculated. The mean value of the coefficient of absorption of the rays in air at atmospheric pressure was found to be

$$0·000279,$$

or the rays would pass through 24·7 metres before absorption reduced the intensity of the radiation to one-half.

The absorption was found to be proportional to the pressure from a half atmosphere to three atmospheres.

The coefficient of absorption in carbonic acid gas was found to be 1·59 times the absorption in air.

Energy required to produce an Ion.

The current produced when a given volume of the gas was ionized by X rays was determined by means of an electrometer. In order to get rid of the secondary radiations set up when X rays strike on a conductor, the rays passed between two charged parallel plates without striking them. A guard-ring method was employed to ensure uniformity of the electric field.

The value of the ionic energy was deduced from the determination of the current, heating effect, and absorption of the rays. The mean value of the energy required to produce an ion in air at atmospheric pressure and temperature was found to be

$$1{\cdot}90 \times 10^{-10} \text{ erg.}$$

This value is much greater than the energy required to produce hydrogen and oxygen ions in the decomposition of water.

The ionic energy of air was found to be approximately the same from pressures of one-half to three atmospheres.

The method of determining the ionic energy for other gases is described, and the evidence that the "ionic energy" is the same for all gases is discussed.

Distance between the Charges of the Ions in a Molecule.

On the assumption that the energy absorbed in producing an ion is due to the work done in separating the ions against the forces of their electrical attraction, it can be shown that the mean distance between the charges of the ions in the molecule is

$$1{\cdot}1 \times 10^{-9} \text{ cm.}$$

This is only $\frac{1}{30}$ of the probable diameter of the atom. This result is in accordance with the view recently advanced by J. J. Thomson, that ionization is produced by the removal of a negative ion from the molecule, and that the negative ion is only a small fraction of the mass of an atom.

Minimum Potential required to produce a Spark.

If the production of ions is necessary before a spark can pass, it can readily be deduced from the value of ionic energy that a spark cannot pass for a potential difference less than 175 volts. Experiments have shown that the minimum value is over 300 volts. The theoretical value is of the same order, but from the complexity of the phenomena a very close agreement could not be expected.

Emission of Energy from Radio-active Substances.

The velocity of the ions produced by Röntgen and uranium radiation in air has been shown to be the same. The ions are thus probably the same, and it is a reasonable assumption that the same energy is required in both cases to produce them. On this assumption the energy radiated by the radio-active substances can be determined.

The radio-active material was spread over a known area and the maximum current produced between the parallel plates determined. The number of ions produced, and consequently the energy to produce them, can be calculated.

For a thick layer of uranium oxide (3 6 grammes spread over a surface of 38 cm.) the energy radiated into the gas for 1 sq. cm. of the surface is

$$10^{-11} \text{ calorie per second.}$$

This amount of energy would suffice to raise 1 c.c. of water 1° C. in 3000 years, assuming no loss of heat by radiation. From observations on the current due to a very thin layer of uranium oxide it is shown that the energy radiated into the gas is not less than 0·032 calorie per year for every gramme of the substance.

The energy radiated from thorium and radium is also considered, and the presence of the rays from radium deflected by a magnet is taken into account.

In the case of radium, which is 100,000 times more radio-active than uranium, the emission of energy per gramme of the substance is not less than 3000 calories per year.

"On Expressed Yeast-cell Plasma (Buchner's 'Zymase')." By ALLAN MACFADYEN, M.D., G. HARRIS MORRIS, Ph.D., and SYDNEY ROWLAND, M.A. Communicated by Sir HENRY E. ROSCOE, F.R.S. Received June 19—Read June 21, 1900.

(First communication.)

Introduction.—In 1897 a communication was published by Professor E. Buchner* in which he described a method by means of which he claimed to have isolated for the first time the active alcoholic ferment from the yeast-cell and to have demonstrated its action upon fermentable sugars. Since then Buchner, mainly in conjunction with Rapp, has from time to time given an account of his further investigations in this direction, and these investigations are still in progress.

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* 'Berichte d. deutsch. Chem. Ges.,' 1897, p. 117. *Vide* also succeeding papers, 1897—1900, *ibid.*

These further investigations, Buchner considers, are confirmatory of the conclusion drawn by him from his original experiments, viz., that the activity of the yeast-cell as an alcoholic ferment depends upon the action of a soluble enzyme of an albuminoid character elaborated by the living cell To this soluble ferment Buchner applies the name "Zymase."

The subject presented so many phases not only of special but also of general biological interest that we were led to pursue its investigation. We considered this the more necessary since Buchner's experiments were carried out entirely with bottom-fermentation yeasts, and it appeared of interest to ascertain whether top fermentation yeasts (as used in English brewing) give parallel results. At the outset we carefully adhered to Buchner's method of expressing the cell plasma; but owing to the tediousness of the process we were led, after many attempts, to adopt the following arrangement for the extraction of the cell plasma or juice.

Method of Preparation of the Cell Plasma.—The yeast as received from the brewery is a thick, pasty, frothy mass, consisting of yeast-cells intermixed with more or less fermented wort. For the purpose in view it is necessary to separate the yeast-cells from all adherent matter which would by its presence influence the composition of the expressed juices. The purification of the yeast is thus a necessary preliminary operation, and is accomplished as follows:—

To the pasty mass of crude yeast is added an equal part of water and the mixture stirred together. This suspension of yeast-cells is then centrifugalised, whereby the contained cells are separated as a thick creamy mass at the bottom of the containing vessel The supernatant liquor is decanted and the mass of cells again stirred into a suspension in a fresh quantity of water. The mixture is again centrifugalised, and the process repeated until the last added water comes away clear and colourless. The final product of this process is a firm mass of yeast-cells closely packed together, with a minimal quantity of adherent water. It is necessary to remove even this quantity of water if a natural juice is to be obtained. The pasty mass of yeast is wrapped in a double thickness of "hydraulic chain cloth" and introduced into one of a series of shallow iron trays, so constructed that the pile can be strongly compressed in a hydraulic press and the expressed liquor run off. In this process, which is a modified form of filter-pressing, the last adherent portions of water are removed from the yeast-cell, and the mass of yeast as removed from the cloths appears as a perfectly dry white powder, consisting of yeast-cells with approximately dry exteriors. The pressure necessary to produce this result is from 70—100 atmospheres.

The disintegration of the yeast-cell is the next process. This is accomplished by a mechanical contrivance which maintains the yeast, together with a proportion of added silver sand, in a condition of violent agitation, in such a manner that in the rapidly succeeding mutual impacts of yeast-cell and sand-particle the cell wall is ruptured and the contents expelled *

If the dry mass of yeast and sand be watched while disintegrating it will be seen to become rapidly pasty, and through successive stages of viscidity it finally reaches a perfectly fluid condition. A microscopical examination at the end of the process fails to discover any whole cells. During the continuance of this process, and in fact during the whole time that elapses between the rupture of the first cell wall and the examination of the final product, the material is kept cool by means of a brine circulation. The brine is maintained at a temperature of - 5° C. by means of expanding anhydrous ammonia. This suffices to keep the disintegrating mass at about 15° C. If this precaution is not adopted the temperature will rise to nearly boiling point, owing to the mechanical production of heat by the impacts and friction of the disintegrating mass.

It now remains to separate the escaped intracellular juices from the suspended cell walls. This is accomplished by a repetition of the same process by which the adherent water was removed from the original yeast. To reduce the mass to a consistency capable of being dealt with by the press, kieselguhr is added (Buchner uses this substance together with sand for grinding). The addition of this substance also serves as a filtering material, and allows the expression from the doughy mass—as from a sponge—of a perfectly clear opalescent product in which no suspended particles can be discovered. A pressure of from 200—300 atmospheres is requisite to express the contained fluid. Such are the main outlines of the method which has been adopted in the preparation of the juice on which the observations that follow were made.

It may be useful to give figures representing the method as it operates in practice on an averagely successful preparation.

From 100 grammes of dried and pressed yeast will be obtained from 30—35 c.c. of expressed juice. The weight of sand employed for grinding will be 100 grammes, and the weight of kieselguhr necessary to reduce the ground mass to a suitable consistency for pressing will be about 80 grammes. The specific gravity is usually from 1050—1060, and the time necessary to completely disintegrate the above quantity of dried yeast is usually $3\frac{1}{2}$ hours.

The physical properties of the juice correspond closely with those described as characteristic by Buchner. The contained proteolytic

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* The precise details of this process, which has been successfully employed for the disintegration of micro-organisms, internal organs, glands and muscle fibres, will form the subject of a separate paper.

enzyme was of a very active character, and produced a rapid digestion of the proteid constituents of the juice.

It occasionally happens that great reluctance is displayed by the juice in leaving the kieselguhr sponge when under pressure. This has most frequently happened with very new yeasts—that is, yeast skimmed from the fermentation vats and used directly for the preparation of the juice. There is some evidence to lead us to suppose that this difficulty is correlated with a similar difficulty which is met with when attempting to prepare as near the living condition as possible an intracellular juice of an organ or tissue. For instance, a liver removed from a dog at the moment of death and at once disintegrated will yield no juice on pressing, even if the pressure be raised to a thousand atmospheres or more, whereas a liver not so fresh will yield its juice without difficulty. That kieselguhr has the power of arresting the passage of certain albuminous bodies can easily be demonstrated. Thus we have found that egg globulins are almost entirely retained in a kieselguhr sponge, and even albumin and serum proteids are retained to a certain extent. It therefore is suggested that the juice that was used in the following work was in every case far removed in nature from the condition in which it existed when alive within the yeast-cell; but on the other hand it is much nearer the living condition than that obtained by Buchner, owing to the fact that he employed water to extract his juice, and water as will be shown has a decided action on the juices we obtained.

We are therefore placed in the difficult position that those conditions in which the juice is nearest its living condition are just those when it cannot be obtained by the convenient method of pressing. Under such circumstances resort must be had to centrifugalising; but the process is tedious in the extreme, and by the time it is completed in all probability the juice has altered in composition. We hope soon to be in a position to overcome this difficulty.

Properties of the Cell Plasma.—In the course of our experiments we employed yeast from five different breweries, which we will designate as A, B, C, D, and E. The first three (A, B, and C) were breweries in the London district, D was in the south of England, and E was one of the very few bottom-fermentation breweries in this country. The greater number of our experiments were made with yeasts from A and B, the yeasts from C, D, and E being only used in one or two instances.

From the outset we found that in practically all cases the juice obtained from the yeasts freely evolved gas, both when standing alone and with the addition of sugar. Our results in the latter respect were fully equal to, and in some instances surpassed, those of Buchner.

We were, however, early confronted with the fact that the *auto-* or *self-fermentation* of the juice gave rise to a considerable volume of gas, a volume which in many cases exceeded that given by the same amount of juice to which sugar had been added. This auto-fermentation apparently escaped the observation of Buchner, who only incidentally refers to it in one of his papers, and who does not appear to have made any correction for the gas evolved from the juice itself in any of his experimental results The extent to which this fermentation occurs may be seen from the subsequent tables (Tables I and II), in one experiment, for instance, 100 c.c. of the fresh juice gave no less than 2·98 grammes or 1500 c.c. of carbon dioxide. This spontaneous evolution of gas takes place even when the juice is kept at a temperature sufficiently low to maintain it in a solid condition. In all probability the gas which Buchner mentions as being evolved on heating "Zymase" is due to this cause.

In our earlier experiments we determined the carbon dioxide evolved from the juice alone, or from its admixture with sugar, by measuring the volume of saturated salt solution which was displaced by the gas, but later we adopted a modification of Hart's double titration method, in which the carbon dioxide was absorbed by sodium hydroxide solution, and the amount determined by double titration.

In the experiments on the relationship of the carbon dioxide and alcohol formed, we absorbed the carbon dioxide evolved in 33 per cent. potassium hydroxide solution contained in Mohr's potash bulbs The alcohol formed in these experiments was estimated by distillation and determination of the specific gravity of the distillate, the weight of absolute alcohol corresponding to the gravity of the distillate being then found by reference to spirit tables.

Control experiments were made in all cases—that is to say, when we were determining the amount of carbon dioxide or of alcohol, formed by any juice from sugar, a corresponding quantity of the juice was placed under identical conditions, but without the addition of any sugar, and the amount of gas evolved or of alcohol formed was carefully determined by the same methods as those used in the experiments in the presence of sugar. We employed antiseptics to inhibit the possible action of yeast-cells or other micro-organisms, the nature of the antiseptics used depending on the object of the experiment. The antiseptics principally employed were sodium arsenite, toluol, and thymol, all at the rate of 1 per cent.

In our earlier experiments we employed 40 per cent. of cane-sugar, this being the concentration which Buchner first considered the most favourable; but we subsequently reduced this to 10 per cent., as we found that greater action was obtained with the lesser concentration. In fact, the larger amount of sugar appeared to exercise a retarding influence on the activity of the juice.

Nature of Results obtained.—In Table I we give the results of some of our experiments, in which the gas evolved was measured by the displacement of salt solution The volume of gas evolved is expressed in this and the following tables on 100 c.c. of juice, although the quantity actually used was, as a rule, either 25 to 40 c.c.

It will be noticed that in nearly every instance more gas was obtained from the auto-fermentation of the juice than from the fermentation in the presence of cane-sugar. This was usually the case with the juice from the yeasts of A, C, and D breweries; how far it is due to the distinctive character of the yeasts or to the high sugar concentration we are at present not prepared to say. Another point to be noted is the great variation in the activity of the juice from different samples of yeast. We noticed this throughout the whole of our experiments, but we are unable to correlate it with any of the physical properties of the juice, such as gravity, &c. It will also be seen that by far the greater part of the action is at an end after twenty-four hours, there being either no further increase in the amount of gas evolved, or comparatively little This we also found common to the majority of our experiments, as will be seen from subsequent tables.

Table I.—Volume of Carbon Dioxide evolved by Cell Juice from different Yeasts, with and without the addition of Cane-sugar.

Source of yeast.	Age of yeast from collection	Carbon dioxide from 100 c c. of juice			
		After 24 hours		After 48 hours.	
		Alone	With 40 per cent. sugar	Alone	With 40 per cent. sugar.
		c.c	c c	c c	c.c.
A*	Fresh	520	280	600	472
A*	1 day	240	808	—	—
A*	2 days	138	164	—	—
A*	Fresh	308	186	308	324
A*	Fresh	400	228	400	340
A*	1 day	285	270	285	320
A*	1 day	162	84	162	150
A†	2 days	990	234	990	290
A†	Fresh	90	170	—	—
A†	3 days	760	560	788	592
A‡†	Fresh	900	2165	—	—
A*	5 days	280	220	—	—
C*	1 day	550	180	—	—
C†	1 day	540	100	—	—
C*	3 days	160	200	—	—
A*	7 days	120	200	—	—
A*	1 day	65	90	—	—

* Toluol used as antiseptic. † Sodium arsenite used as antiseptic.

‡ In this experiment 10 per cent. cane-sugar was employed.

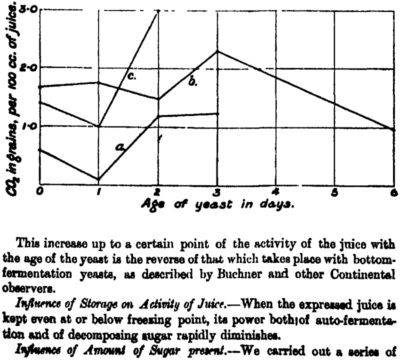
Table II —Influence of Age of Yeast on Activity of Juice

Source of yeast	Age of yeast	Gas evolved per 100 c c of juice							
		After 24 hours				After 48 hours			
		Alone	5 per cent C S	10 per cent C S	15 per cent C S	Alone	5 per cent C S	10 per cent C S	15 per cent C S
		grammes	grammes	grammes	grammes	grammes	grammes	grammes	grammes
A	Fresh	0 215	0 145	—	—	0 615	0 3[illegible]5		—
	1 day	0 105	0·015	—	—	0 120	0 200		—
	2 days	0 930	0 445	0 395	0 140	1 205	0 965	0 855	0 5[illegible]0
	3 days	1 145	—	0 230	—	1 23	—	0 950	
B	Fresh	1 480	3 030	3 870	—	1 705	3 550	5 060	
	1 day	1 450	3 140	3 7[illegible]0	3 630	1 [illegible]0	3 590	4 600	4 050
	2 days	1 480	3 540	4 410	3 880	1 480	3·920	5 300	3·870
	3 days	—	—	—	—	2 310	2 180	6 600	3 300
	6 days	0 83		1 3[illegible]0	3 230	0 9[illegible]0	—	1 950	3 [illegible]0
B	Fresh	0 90	2 800		3 [illegible]20	1 420	2 [illegible]30	—	4 450
	1 day	0·97	2 040	4 040	3 530	1 [illegible]3[illegible]	2 930	4 440	4 1[illegible]0
	2 days	1 32	2 990	3 390	3 150	2 9[illegible]0	3 290	3 [illegible]00	3 000

Influence of Age of Yeast on Activity of Juice.—In Table II we give some of our results on the influence of the age of the yeast, *i.e.*, the time which elapsed between the time of collection of the yeast in the brewery, and that of pressing and grinding, on the activity of the cell plasma obtained. The same table shows to some extent the influence of sugar concentration on the amount of gas evolved.

It will be seen that the results are very variable, but that the general tendency is for the activity of the juice to increase to a certain point with the age of the yeast, the maximum being reached about the 3rd or 4th day from collection. After the maximum is reached there is a very rapid decline in the activity of the juice. The variation in the amount of auto-fermentation is not so great, but the tendency of this is to follow the same direction The results are shown diagrammatically in figs. 1 and 2, the former showing the auto-fermentation and the latter the fermentation in presence of sugar, both when the gas evolved from the auto-fermentation is included and when it is deducted from the total amount.

FIG 1.—Showing the influence of age of yeast on auto-fermentation after 48 hours

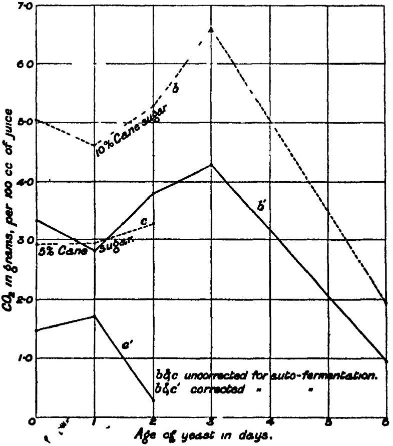


This increase up to a certain point of the activity of the juice with the age of the yeast is the reverse of that which takes place with bottom-fermentation yeasts, as described by Buchner and other Continental observers.

Influence of Storage on Activity of Juice.—When the expressed juice is kept even at or below freezing point, its power both of auto-fermentation and of decomposing sugar rapidly diminishes.

Influence of Amount of Sugar present.—We carried out a series of experiments to determine the most favourable concentration of sugar, the results show that the smaller amounts—5 to 10 per cent—give the most favourable results, whilst the larger quantities sensibly retard the action, i.e., less gas is obtained from the juice plus sugar than from the juice alone This probably explains to some extent the results we

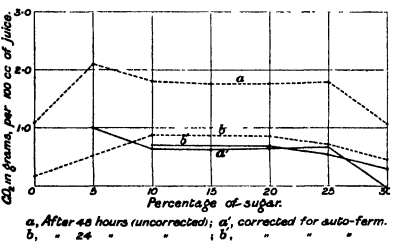
FIG 2.—Showing the influence of age of yeast on the activity of expressed juice after 48 hours



obtained in our earlier experiments in which 40 per cent of sugar was employed Some of the results are shown diagrammatically in fig 3

Influence of different Sugars.—In order to determine if the nature of the sugar employed had any influence on the amount of gas evolved, we carried out a series of experiments with cane-sugar, dextrose, maltose, and levulose at different concentrations, using the same sample of juice for each set of experiments. The results, as a whole, show that more carbon dioxide is given off from cane-sugar than from either of the other sugars.

FIG. 3.—Showing the influence of different concentrations of sugar on gas evolved.



Influence of Temperature.—We made several experiments to ascertain the most favourable temperature for the action of the juice. As an example of the results obtained, we may quote the following:—

The juice was mixed with 10 per cent. of cane-sugar in the usual way, and there were obtained—

At 0° C.	... 0·41 gramme	of carbon dioxide	in 48	hours.
,, 10	...... 0·83	,,	,,	,,
,, 25	.... 1·05	,,	,,	,,
,, 37	.... 1·17	,,	,,	,,

The higher temperatures therefore appear to increase the activity of the juice.

Influence of Filtration.—In order to ascertain what influence, if any, filtration through Chamberland and Berkefeld filters had on the activity of the juice, we carried out a series of experiments with different juices, carefully testing their gas-producing activity before and after filtration. Thymol was used as an antiseptic in each case. The results are given in Table III, and it will be seen that filtration decreases to a considerable extent, but without entirely destroying, both the auto-fermentation and the action of the juice on sugar. This decrease in gas-evolving power is accompanied by a very considerable fall in gravity of the juice. These experiments agree with those of Buchner on the same point.

Table III.—Influence of Filtration on the Activity of the Juice.

Source of yeast	Age of yeast	Gravity before filtration.	Gravity after filtration.	Gas evolved from 100 c.c. of juice.			
				Before filtration		After filtration	
				Alone.	With 10 per cent C.S.	Alone	With 10 per cent C.S.
	days.			gramme.	gramme.	gramme.	gramme.
B*	3	—	—	—	0·67	—	0·31
B	2	—	—	0·43	0·65	0·00	0·10
B	2	–		0·18	1·56	0·25	0·84
B†	2	—	—	1·23	0·83	0·11	0·43
B	2	1055	1018	0·57	0·65	0·24	0·23
B	3	1045	1030	0·25	1·55	0·24	1·17

* In this experiment the filtration was through a Chamberland filter; in the remaining experiments a Berkefeld filter was used.

† After 72 hours in this case; all the others after 48 hours.

Influence of Dilution.—In considering the nature of the action of the juice and of the agent to which the evolution of gas was due, it appeared important to ascertain the effect of dilution on the action of the juice. All experiments were conducted by adding the weighed quantity of sugar to the juice itself, so that no water at all was introduced. If the action were a purely enzymic one, dilution to a limited extent should not appreciably affect the result; whereas if the action were due to other causes, it might be influenced to a greater or less extent. We accordingly carried out a series of determinations on dilution with water alone, with physiological salt solution (0·75 per cent. sodium chloride), and with water in the presence of cane sugar. The experiments with sugar were made in two ways: in the one, the sugar was added to the juice in the usual way (10 per cent.), and water was then added to bring about the desired dilution, the ratio of the sugar to juice being therefore kept constant; in the other, the dilution was made with a 10 per cent. solution of sugar, so that the ratio of sugar to the total volume was maintained throughout. The results obtained are set out in Table IV. An examination of the results will at once show that the auto-fermentation of the juice is greatly influenced by dilution both with water and with salt solution. The addition of an equal volume of water sensibly retards the action.

Table IV —Influence of Dilution on Activity of Juice

Source of yeast	Age of yeast	Gas evolved from 100 c c of juice							
		After 24 hours				After 48 hours			
		Alone	Diluted 1 1	Diluted 1 2	Diluted 1 3	Alone	Diluted 1 1	Diluted 1 2	Diluted 1 3
(a) With water alone									
		grammes	grammes	gramme	gramme	grammes	grammes	gramme	gramme
A	1 day	1 49	1 25	0 02	0 00	1 71	1 32	0 05	0 00
B	5 days	0 83	—	0 04	—	0 97	—	0 58	—
B	5	1 05	—	0 00	—	1 37	—	0 09	—
B	6	1 22	0 94	0 31	0 07	—	—	—	—
(b) With 0 5 per cent salt solution									
A	Fresh	0 90	0 05	0 00	0 00	—	—	—	—
B	5 days	0 83	—	0 10	—	0 9	—	0 17	—
B	5	1 05		0 00	—	1 37	—	0 08	—
(c) With water in presence of 10 per cent cane sugar on juice									
B	6 days	1 30	0 10	0 10	0 05	1 30	0 10	0 10	0 06
B	6	1 66	0 23	0 10	0 08	—	—	—	—
(d) With water in presence of 10 per cent cane sugar on total volume									
B	5 days	1 77	0 67	0 47	0 55	—	—	—	—

and, in some cases, *dilution with a double volume practically stops the evolution of gas*. With salt solution, the action is still more marked.

In the presence of sugar the retarding action is still distinctly apparent, especially when the concentration of the sugar decreases with dilution. In this case the effect of dilution is fully as marked as in the case of water alone or of salt solution. When the strength of the sugar solution is maintained constant, the retardation is still considerable, but not so great as in the other cases.

This paralysing effect of dilution on the activity of the juice is so contrary to the behaviour of enzymes in general under similar conditions, that in our opinion it forms a grave objection to the acceptance of Buchner's enzyme theory. Since the above experiments were made, we find that Wroblewski* has conducted dilution experiments with like results.

In connection with the question of the influence of dilution on enzyme action, it may be mentioned that when a sample of six-day juice was diluted to 1 in 1000 with cane sugar solution, 50·5 per cent. of the cane-sugar was found to be inverted, whilst with another juice, three days old, a dilution of 1 in 100 showed an inversion of 79·5 per cent. of the cane-sugar present. This offers a great contrast to the effect of dilution of the juice on the production of carbon dioxide.

Ratio of Carbon Dioxide to Alcohol.—In connection with the question whether we had to do with a true alcoholic fermentation, it became important to determine if carbon dioxide and alcohol were produced in the proportions ordinarily found, and if the amount of sugar which disappeared during the experiment bore any relation to the alcohol and carbon dioxide. We carried out a large number of experiments with a view to elucidate these points, and the results of some of the experiments are shown in Table V. In experiments 1 to 5, the alcohol and carbonic acid estimations were made on the same fermentation, but in experiments 6 to 15 we carried out duplicate fermentations, under identical conditions with the same juice, for the two determinations. We did this in order to ensure greater accuracy in the alcohol estimation, since the escaping gas could be washed by passage through a little water, which was subsequently added to the distillation flask. When we were estimating both products from the same experiment this was not possible.

It will be noticed from the table that the juice as it comes from the press always contains a considerable amount of alcohol, and we found on examination that this agrees fairly closely with the amount of alcohol contained in the yeast, even after the thorough washing and pressing to which it had been subjected in the preliminary treatment.

When corrections are made for the amounts of alcohol and of carbon dioxide formed during the auto-fermentation of the juice, the ratio

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* 'Centralbl. f. Physiol.,' 1899, p. 284.

Table V —Showing Relation of Alcohol and Carbon Dioxide formed in Fermentations in the Cell juice

The results are expressed on 100 c c of Cell juice

No of expt	Age of yeast	Original alcohol	Auto fermentation			Fermentation with 10 per cent sugar					
			CO_2 formed	Alcohol formed	Increase of alcohol	CO formed	Sugar fermented	Increase of CO_2	Alcohol formed	Sugar fermented	Increase of alcohol
	days	grammes	grammes	grammes	grammes	grammes	grammes	grammes	grammes	grammes	grammes
1	1	1 85	1 38	2 90	1 05	1 64	5 838	0 26	4 75	5 838	1 85
2	2	3 90	1 25	7 0	4 00	0 [illegible]	4 292	none	7 50	4 292	none
3	3	3 25	0 65	3 25	none	2 03	6 101	1 3	6 10	6 101	2 85
4	4	4 20	1 89	5 95	1 75	[illegible] 45	—	0 56	6 5	4 615	0 60
5	6	4 20	0 95	3 15	none	1 16	1 20	0 71	3 40	—	0 25
6	3	3 70	1 11	4 30	0 60	1 22	2 575	0 11	5 5	2 912	0 95
7	5	3 95	1 20	4 80	0 85	1 21	2 181	0 01	5 95	2 830	0 45
8	7	3 80	0 64	3 60	none	1 03	4 115	0 [illegible]	4 25	1 758	0 65
9	7	4 45	0 51	5 05	0 60	1 16	7 413	0 65	5 70	4 390	0 65
10	3	4 12	1 49	4 52	0 40	1 50	3 676	0 01	4 [illegible]	2 107	0 32
11	5	2 90	0 40	3 00	0 10	0 59	1 553	0 19	2 75	1 096	none
12	2	2 80	0 73	3 75	0 95	0 61	1 340	0 15	3 60	4 150	0 15
13	1	2 12	0 23	1 75	0 63	0 18	none	none	1 95	none	0 10
14	3	3 15	0 37	3 12	none	[illegible] 27	5 641	1 90	5 55	6 413	2 48
15	3	4 17	0 56	4 25	0 08	3 66	6 331	3 10	7 50	9 080	3 25

Experiments 1 to 7 and 12 to 15 were made with dextrose Experiments 8 to 11 were made with cane sugar Thymol was used in all the experiments between the residual alcohol and the carbon dioxide is very variable, and only in cases in which a very active juice is employed does the ratio approximate to that found by Pasteur.* With a weak juice there appears to be little or no connection between the two, the amount of alcohol formed being, as a rule, greater than the carbon dioxide. The small quantities of alcohol to be determined may be thought to be accountable for the discrepancy between the two products, but the following example of a determination carried out in duplicate shows that our methods were capable of considerable accuracy.

	I. Gramme	II. Gramme
Carbon dioxide evolved	0·327	0·337
Alcohol formed	0·95	0·95
Sugar fermented	1·167	1·158

One very remarkable fact comes out in all the above experiments, namely, that the amount of sugar which disappears is greatly *in excess* of that actually fermented, as deduced either from the alcohol or from the carbon dioxide formed. The closer the relationship, however, between the two products, the less is the excess of sugar which disappears.

It occurred to us that there might be some constituent of the juice which interfered with the correct determination of the sugar, but we put this to the test and found that when we added sugar to the juice, and then killed its action by heat before any fermentation could take place, the whole of the sugar could be accounted for by Pavy's method of determination.

We also submitted the residual product after fermentation had taken place to hydrolysis with very dilute acid with a view to break up any hydrolysible compound which might have been formed between the constituents of the juice and the excess sugar which had disappeared, but without any result · the reducing power before and after treatment remained the same. The sugar had therefore apparently disappeared as such, and had not simply been rendered unrecognisable to ordinary tests.

We are at present only able to chronicle this most interesting fact, as at the present stage of our work it would be premature to make any theoretical deductions; but in connection with this remarkable disappearance, we venture to throw out the following suggestion:—During the life of the yeast, sugar is consumed by the organism with the resulting production of carbon dioxide and alcohol. Considered in detail, this process probably occurs in two stages—(1) a building up and incorporation of the sugar molecules into the actively living protoplasm (anabolism); and (2) a breaking down of this complex material into simpler products, of which carbon dioxide and alcohol are the constant and principal constituents (katabolism). May it not be that after the expression of the cell-juice from the cell the same series of actions continues to take place, at least for so long a time as the rapidly changing and unstable cell-juice remains in a condition approximately identical with that in which it existed in the living cell? If this hypothesis be admitted, then the varying activities of the juice are at least partly explicable, for if we designate by χ the hypothetical protoplasmic constituent of the cell with which the sugar combines, then we may imagine the processes which take place in the expressed cell-juice (in which we assume χ to continue to exist) to be somewhat as follows:—

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* Cane-sugar yields 51·11 per cent. of alcohol and 49·42 per cent. CO_2.
Dextrose „ 48·55 „ „ „ 46·95 „ „

(*a*) In the case of auto-fermentation the χ sugar combination, built up during the life of the cell, continues to decompose, after the expression of the juice, yielding carbon dioxide and alcohol.

(*b*) In the case of the disappearing sugar, the formation of the χ-sugar combination continues to a certain point, depending on the activity of the juice, but the decomposition of this combination comes to an end before the whole of the sugar has been liberated in the form of carbon dioxide and alcohol. In the case of a very active juice we may imagine this process to continue until practically the whole of the combination has been decomposed. In the case of a weak juice, the building-up process takes place more rapidly than the breaking-down process, and, consequently, when the activity of χ ceases, there remains an excess of sugar in the form of the χ-sugar combination.

We are continuing our investigations with the yeast-cell plasma, and shall hope to communicate our further results to the Society in due course. In the meantime it may be convenient to briefly summarise the results we have already obtained, which so far appear to be leading us in the direction not of an enzyme explanation of the process, but rather of a theory which refers the phenomenon to the vital activity of the yeast-cell protoplasm.

(1.) The top-yeast of English breweries yields, by suitable treatment, a cell-juice which possesses the transient power of decomposing sugar into alcohol and carbonic acid.

(2.) The amount of gas formed by an active juice is as great as, or even greater than, that found by E. Buchner.

(3.) The cell-juice as prepared by us undergoes a very considerable auto-fermentation, in some instances exceeding that given by a mixture of the same juice and cane-sugar.

(4.) A moderate dilution (1 : 2) with water or physiological salt solution practically stops all fermentative activity.

(5.) Only with a very active cell-juice does the ratio between the alcohol and carbon dioxide formed approximate to that found in ordinary alcoholic fermentation.

(6.) When the cell-juice is allowed to act on sugar—either cane-sugar or dextrose the quantity of sugar which disappears is considerably in excess of that which can be accounted for by the production of carbon dioxide and alcohol.

"On the Thermodynamical Properties of Gases and Vapours as deduced from a Modified Form of the Joule-Thomson Equation, with Special Reference to the Properties of Steam."

By H. L. CALLENDAR, M.A., LL.D., F.R.S., Quain Professor of Experimental Physics, University College, London. Received and Read June 21, 1900.

At the present time, the relations between the specific heats and other thermodynamical properties of gases and vapours, and the deviations from the behaviour of the ideal gaseous substance in isothermal and adiabatic expansion, remain extremely obscure. The variation of the latent heat of a vapour, and of its saturation pressure, are generally expressed by purely empirical formulæ, without theoretical foundation. Various equations, such as those of Van der Waals, and Clausius, have been proposed and have been very generally adopted to represent some of the simplest of these relations, but owing to their complexity, and to the number of empirical constants involved, their utility is seriously limited, and the results to which they lead are in some cases undoubtedly erroneous.

The object of the present paper, which is founded mainly on experiments on steam, is to develop the application of a modified form of the Joule-Thomson equation, which is sufficiently simple to be of great value in the discussion of the thermodynamical relations of gases and vapours, and which leads directly to accurate formulæ for many properties which have hitherto been represented empirically

To take the case of steam as an example, all tables of the properties of steam are at present founded on Regnault's formula for the total heat H of saturated steam at *t* C. reckoned from 0° C., namely:

$$H = 606{\cdot}5 + 0{\cdot}305t \quad \ldots\ldots\ldots\ \ldots\ldots\ldots\ldots \quad (1),$$

and on his empirical formula for the pressure of saturated steam, namely:

$$\log p = a + bB^t + cC^t \quad \ldots\ldots\ldots\ldots\ldots \quad (2).$$

The latter formula contains five empirical constants, but it is usual to employ two different formulæ to cover the range 0 —220° C. of his experiments.

The specific volume of saturated steam, owing to the effects of surface condensation,* cannot be determined by direct experiment, and is generally deduced from the above empirical formulæ, by the application of the well-known thermodynamical relation,

$$L/(v - b) = \theta dp/d\theta \quad .. \quad (3),$$

where L is the latent heat, v the specific volume of the saturated vapour, and b that of the liquid, θ the absolute temperature, and $dp/d\theta$ the rate of increase of the saturation pressure with temperature.

Regnault also determined the specific heat of superheated steam at atmospheric pressure by condensing highly superheated steam in a calorimeter; but owing to the small proportion which the superheat of the steam bears to the latent heat, and to the difficulty of calorimetric work at high temperatures, the measurements were not very certain, and many recent experimentalists and writers (*e.g.*, Ewing, Perry, Grindley) have preferred to adopt widely different values deduced by other methods from the formula for the total heat. It was proved by Rankine† that the rate of change of the total heat of steam at low temperatures, at which it very nearly follows the laws of an ideal gas on account of its low pressure and large specific volume, must be very nearly equal to the specific heat of the vapour at constant pressure. Therefore either the specific heat of steam at low temperatures must be 0·305, increasing considerably with the temperature so as to reach the value 0·48 between 100 and 200 C., or else the observations of Regnault must be wrong. In any case it is clear that the variation of the total heat should not be linear, unless we abandon the experimental evidence in favour of the constancy of the specific heat of an ideal gas. It is most likely that the source of the discrepancy is to be found in the difficult calorimetric measurements of the rate of change of the total heat at low temperatures. The determinations of the latent heat by Griffiths, 572·6 calories at 40·2° C., and by Dieterici, 596·7 calories at 0° C.,‡ are from 6 to 10 calories smaller than Regnault's, and imply a rate of change of total heat about 30 per cent. larger, and more nearly equal to the theoretical value. At temperatures above 100° C., the determinations of Regnault are more consistent, but it is very likely, from the method which he employed, that they may be considerably in error. His observations show a sudden increase of six calories above 175° C., which is explained by the discovery and rectification of a leakage of steam through the distributing tap into the idle calorimeter. It is clear that he regards the observations above this point with greater confidence. In any case it is unlikely that the order of accuracy attained in his experiments was greater than one-half of 1 per cent at any point, because his thermometers were not sufficiently perfect, and because it is practically certain from the recent determinations of Reynolds and Moorby of the mean specific heat of water between 0° and 100° C., and from the work of Callendar and Barnes on the variation of the specific heat over the whole range 0° to 100° C., that his value for the specific heat of water at 100° C. is at least 1 per cent. too large. He was also ignorant of the considerable changes which occur in the specific heat of water at low temperatures, and it is evident that his work, though far in advance of his time, requires revision when considered in the light of the great advances which have been made in the last fifty years.

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* Ramsay and Young, 'Phil. Trans.,' A, 1892.

† 'Roy. Soc. Edin. Proc.,' 1850.

‡ Griffiths, 'Roy. Soc. Proc.,' December, 1894.

It is obvious from the nature of the problem that the most appropriate method of determining, either the variation of the total heat of steam, or the specific heat of steam, is by the application of some *differential* method, which shall be independent of the determination of the latent heat. In the papers which follow, I have described the application of two such methods to the case of steam. By means of the "Differential Throttling Calorimeter" it is possible, following the method of Joule and Thomson, to determine accurately the variation of the total heat of steam, and the deviations of the specific volume from the ideal gaseous state, in terms of the specific heat at constant pressure. By the "Electrical Method of Measuring the Specific Heat," which is exactly similar to the method already applied* in the case of water, it is possible to determine the specific heat without reference to the latent heat. The details of these experiments are reserved for subsequent communications, the object of the present paper is to explain the thermodynamical relations involved, and to exhibit the calculation of the variations of the specific volume, the specific heats, the total heat, the latent heat, and the pressure of saturated steam in terms of the quantities which are directly observed. The theory of the method is applicable, and has been already applied, to some problems connected with gases, but in dealing with vapours some additions are required, and it is clear that the original equation of Joule and Thomson requires some important modifications.

Modification of the Joule-Thomson Equation.

In order to represent the observations of Regnault on the deviations CO_2 from Boyle's law, Rankine in 1854 proposed the equation,

$$pv = R\theta - a/\theta v \quad \text{.........................} \quad (4).$$

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* Callendar and Barnes, 'Brit. Assoc. Report,' 1897 and 1899.

If we write $(v - b)$ for v, on the left hand side, this equation is practically identical, for moderate pressures, with the modified form of the equation of Van der Waals which was devised by Clausius* to meet the objection that the term a/v in the equation of Van der Waals did not satisfactorily represent the variation of the phenomena with temperature In adopting this equation of Rankine's to represent their observations "On the Thermal Effects of Fluids in Motion,"† Joule and Thomson substituted $v = R\theta/p$ in the small term $a/\theta v$, which thus became $ap/R\theta^2$. For the purpose of their observations the modification appeared to be unimportant, and they quote the equation as Rankine's, but it really introduces a great simplification. If we write the equation in the form,

$$v - R\theta/p = a/R\theta^2 \quad . \; . \; . \; . \; . \quad (5),$$

we observe that the small term is a function of the temperature only, and is independent of the pressure or volume The isothermals on the p, v diagram are equilateral hyperbolas, identical in form with those of an ideal gas. Or, if we plot the product pv against p, as is usual in *considering the deviations of a gas from* Boyle's law, the *isothermals* are straight lines inclined to the axis of p at various angles, which diminish as the temperature rises. It is proved by the experiments of Joule and Thomson, and more clearly by the subsequent observations of Amagat and others, that the equation, even in this simple form, represents a very good first approximation to the deviations of actual gases from Boyle's law at moderate pressures. The approximation holds, for instance, in the case of CO_2, according to the observations of Amagat, up to 50 or 100 atmospheres at temperatures between 100° and 200° C The application of the equation to the case of vapours may, however, be still further simplified, and rendered at the same time more accurate, by two slight but important modifications.

(1) It is practically certain that the equation of a perfect, or pluperfect, gas at high temperatures is not $pv = R\theta$, but $p(v - b) = R\theta$, where b is the minimum volume or "co-volume" of Hirn and Van der Waals. The co-volume b is variously regarded as being equal to four times or $4\sqrt{2}$ times the absolute volume of the molecules. It is relatively small at moderate pressures (about one-thousandth of v at atmospheric pressure), and is often negligible, but may with great probability be taken as equal to the volume of the liquid at temperatures where the vapour pressure is small.

(2) It is usual in the kinetic theory of gases, either tacitly or explicitly, to make the fundamental assumption that the average total kinetic energy of the molecules of a gas, including motions of vibration and rotation, is directly proportional to the kinetic energy of transla-

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* 'Phil. Mag.,' June, 1880.

† 'Phil. Trans.,' 1862.

tion, which is equal to $3pv/2$ per unit mass at any temperature. It follows from this assumption that the limiting value of the specific heat of a gas in the ideal state ($p = 0$, $v = \infty$), either at constant pressure or at constant volume, must be constant, if the molecule is stable, since it is directly proportional to pv/θ, which tends to a constant limit when $p = 0$, even in the case of vapours at temperatures far below their boiling points. These constant limiting values of the two fundamental specific heats will be denoted by the symbols S° and s° respectively. As a further simplification we may assume that the kinetic energy of a vapour is proportional to $p(v-b)$ at all stages and not only in the limit. On this assumption it is also necessary to suppose that the index of θ in the small term $a/R\theta^2$ in the Joule-Thomson equation is not 2, but $n = s^\circ/R$, the ratio of the limiting value of the specific heat at constant volume to the limiting value of pv/θ. If we adopt the hypothesis of Clerk Maxwell with regard to the distribution of energy between the various degrees of freedom of a molecule, which, in the absence of certain knowledge with regard to the exact nature of a molecule, appears to be the only practical working hypothesis, the theoretical value of this limiting ratio should be 1·5 for a monatomic gas like argon, 2·5 for a diatomic gas like oxygen or hydrogen, 3·5 for a triatomic gas like steam or CO_2, and so on, increasing by unity for each additional atom in the molecule. The value 3·5 for the index is closely verified in the case of steam by the experiments to be described on the Joule-Thomson effect, and also by the experiments on the specific heat, by which this relation was first suggested.

Adopting these two modifications, of which the second is the more important, the equation may be written in the form,

$$v - b = R\theta/p - c^\circ(\theta^\circ/\theta)^n = V - c. \quad \ldots \quad \ldots. \quad (6),$$

in which V is taken as a convenient abbreviation for the ideal volume $R\theta/p$, and the co-volume b is taken as constant and equal to the volume of the liquid. The small correction c, representing the state of co-aggregation of the molecules, is called the "co-aggregation volume," and is a function of the temperature only, varying inversely as the nth power of the absolute temperature, where the index n is used as an abbreviation for s°/R. It is a quantity of the same dimensions as a volume, and is measured in cubic centimetres. The numerical value of c in the case of steam at 100° C. is 26·5 c.c., as deduced from the experiments on the Joule-Thomson effect and the specific heat. The calculated value of c° at $\theta^\circ = 273·0°$ is 79·0 c.c. It is obvious on the simplest considerations that the co-aggregation volume c cannot remain accurately constant at high pressures, since there is an obvious limit to the possible co-aggregation of the molecules. If, for instance, the molecules are simply paired, the pairing must cease when $v - b = c$. But it is certain from the differential experiments that the modified equation represents a very accurate approximation to the facts at moderate pressures, although n is not necessarily equal to s°/R in all cases.

Variation of the Specific Heats.

It would appear at first sight as though the modified equation were more complicated than the original of Joule and Thomson, but it leads as a matter of fact to far simpler relations between the thermodynamical properties, and makes it possible to attack problems which would be quite intractable with the more complicated forms of empirical equations in vogue.

If S and s are the specific heats at constant pressure and at constant volume respectively, and ϕ is the entropy, we have the well-known relations,

$$(dS/dp)_\theta = \theta d^2\phi/d\theta\, dp = -\theta(d^2v/d\theta^2)_p \ldots \qquad (7),$$

$$(ds/dv)_\theta = \theta d^2\phi/d\theta\, dv = +\theta(d^2p/d\theta^2)_v \qquad (8).$$

Assuming the characteristic equation (6), it is easy to prove from these relations that the values of the specific heats at any temperature and pressure are given by the simple formulæ,

$$S = S^\circ(1+nc/V) = S^\circ + n(n+1)pc/\theta \qquad (9),$$

$$s = s^\circ(1+nc/V)(1-c/V) \ldots \ldots \ldots (10),$$

where S° and s° are the constant limiting values of the specific heats when $p = 0$. The ratio of the specific heats $g = S/s$ is given by the relation,

$$g = S/s = S^\circ/s^\circ(1-c/V) = g^\circ/(1-c/V) \ldots \ldots (11),$$

where g° stands for the constant limiting value of the ratio, and is equal to $(n+1)/n$.

Isentropic Relations.

The isentropic relations are greatly simplified by the assumption $n = s^\circ/R$, since in this case the ratio of the co-aggregation volume c to the ideal volume V, or to the difference of specific volumes of the vapour and liquid $v-b$, is constant at constant entropy. From the characteristic equation (6), we have for the isothermal elasticity E_θ and the isentropic elasticity E_ϕ, since $E_\phi/E_\theta = S/s = g = g^\circ/(1-c/V)$,

$$E_\theta = -v(dp/dv)_\theta = +pv/V \ldots\ldots \ldots \ldots\ldots \qquad \ldots\ldots (12),$$

$$E_\phi = -v(dp/dv)_\phi = +gE_\theta = g^\circ pv/(v-b) \ \ldots\ldots (13).$$

The equation of the isentropics may therefore be written in the forms—

$$p(v-b)^{g^\circ} = \text{constant}, \quad \text{or} \quad p^n(v-b)^{n+1} = \text{constant},$$

$$\text{or} \qquad p/\theta^{n+1} = \text{constant}, \quad \text{or} \quad (v-b)\theta^n = \text{constant} \ldots\ldots\ldots (14).$$

The constant value of the ratio c/V along an isentropic is given by the relation,

$$e^{nc/V} e^{\phi/R}/V = e^{nc^\circ/V^\circ} e^{\phi^\circ/R}/V^\circ \text{ .... } (15),$$

which may be written in the form,

$$c/V = (c^\circ/V^\circ)(p/p^\circ)(\theta^\circ/\theta)^{n+1} \text{ . . .. } (16),$$

where e is the base of natural logarithms, and the symbols p°, c°, V°, θ°, ϕ°, refer to a standard state such as 0° C., and 760 mm.

Isothermal Expansion

The Change of Entropy $\phi - \phi^\circ$ in expansion from p° to p at a constant temperature θ° is deduced from the relation,

$$d\phi = (d\phi/dp)_\theta dp = -(dv/d\theta)_p dp = -(R/p + nc^\circ/\theta^\circ)dp \text{ .. } (17),$$

whence
$$\phi - \phi^\circ = R\log p^\circ/p + (p^\circ - p)nc^\circ/\theta \text{ . ... } (18).$$

The Heat Absorbed is given by the expression,

$$Q - Q^\circ = R\theta \log p^\circ/p + (p^\circ - p)nc^\circ \text{ .... } (19).$$

The Work Done, $W = R\theta \log p^\circ/p$, is the same as for an ideal gas between the same limits of pressure.

The Change of Intrinsic Energy is, therefore,

$$E - E^\circ = nc^\circ(p^\circ - p) \text{ .... } (20).$$

Heating at Constant Pressure.

The Heat Absorbed in a rise of temperature from θ° to θ at constant pressure p° is given by the equation

$$Q - Q^\circ = \int S d\theta = S^\circ(\theta - \theta^\circ) + (n+1)p^\circ(c^\circ - c) \text{ .... } (21).$$

Change of Energy,

$$E - E^\circ = s^\circ(\theta - \theta^\circ) + np^\circ(c^\circ - c) \text{ .... } (22).$$

The Work Done,

$$W = R(\theta - \theta^\circ) + p^\circ(c^\circ - c) \text{ .... } (23).$$

Change of Entropy,

$$\phi - \phi^\circ = \int S d\theta/\theta = S^\circ \log \theta/\theta^\circ + np^\circ(c^\circ/\theta^\circ - c/\theta) \text{ ...... } (24).$$

In passing from any one state represented by the co-ordinates p°, θ°, to any other state represented by p, θ, both the change of energy and the change of entropy must be independent of the process by which the change is effected, and equal to the values calculated by combining isothermal expansion with heating at constant pressure. We thus obtain the following general expressions for the change of energy and the change of entropy in any transformation from the state p°, θ°, to the state p, θ —

$$\mathrm{E}-\mathrm{E}^{\circ} = s(\theta-\theta^{\circ}) - n(pc-p^{\circ}c^{\circ}) \quad . \quad .. \quad ... \quad (25).$$

$$\phi-\phi^{\circ} = \mathrm{S}^{\circ}\log\theta/\theta^{\circ} - \mathrm{R}\log p/p^{\circ} - n(cp/\theta - c^{\circ}p^{\circ}/\theta^{\circ}). \quad ... \quad (26).$$

These expressions are true for any value of n.

Calculation of the Specific Volume of Saturated Steam.

As an illustration of the numerical application of this method, I propose to take the case of steam, as the most important and interesting. But the methods and reasoning would be equally applicable to any other gases or vapours for which the requisite experimental data were available

The deviations of the specific volume from the ideal state are immediately given by the values of the co-aggregation volume c, which are easily calculated. It is quite a mistake to suppose, as is frequently stated, that there is any sudden or rapid change in the co-aggregation as the saturation point is approached. This idea has arisen merely from experimental errors due to surface condensation. Under certain conditions it is well known that the vapour can exist in stable equilibrium at pressures greatly in excess of the saturation value, provided that there is no liquid present, or any nuclei, or other aids to condensation. I have not, for obvious reasons, succeeded in investigating the properties of *supersaturated* steam by the method of throttling; but there does not appear to be any reason to suppose that its behaviour could not be predicted with great probability by assuming that the co-aggregation volume remains constant at constant temperature, which is certainly a very close approximation to the behaviour of steam in the superheated condition down to the temperature of saturation.

In the following table, which contains a few sample values of the co-aggregation c and the specific volume v, the ideal specific volume of steam at 100° C. and 760 mm. pressure is taken as 1698·0 c.c., which is calculated by assuming the density of oxygen, corrected for its probable co-aggregation, and taking the ratio of the molecular weight of steam to that of oxygen to be 18/32. The value of c for steam at 100° C. is taken as 26·5 c.c., and the values at other temperatures are calculated by the formula (6):—

Table I.—Specific Volume and Co-aggregation of Steam.

Temp Cent.	Saturation-pressure	Co-aggregation c	Ideal volume. V	Specific volume. v	Co aggregation Ratio c/V	Deviation. Ratio. $(c-b)/V$.
°	atmos	c c	c c	c c		
0	0·00613	79·0	202680·0	202602·0	0·000389	0·000385
20	0·02323	61·7	57370·0	57309·0	0·001076	0·001000
40	0·0731	49·0	19490·0	19442·0	0·002515	0·002468
60	0·1267	39·4	7710·0	7671·0	0·00512	0·00498
80	0·4670	32·14	3438·0	3407·0	0·00932	0·00903
100	1·0000	26·50	1698·0	1672·5	0·01560	0·01500
120	1·961	22·07	911·7	890·6	0·02425	0·02305
140	3·570	18·56	526·0	508·4	0·0354	0·0333
160	6·01	15·74	321·7	307·1	0·0490	0·0455
180	9·93	13·41	207·7	195·8	0·0648	0·0594
200	15·37	11·55	140·1	129·6	0·0825	0·0742

The values of the specific volume v are calculated for the saturation-pressures, given in atmospheres in the second column. The values for any other pressures can be calculated with equal ease, since the co-aggregation-volume c, which is given in the third column, depends only on the temperature. It is necessary in each case to calculate the appropriate value of the ideal volume $V = R\theta/p$. The values of the specific volume v are found by subtracting $(c-b)$ from the ideal volume. The values of the co-aggregation ratio c/V, and the deviation-ratio $(c-b)/V$, are also given, as the first is useful in calculating the values of the specific heats by formulæ (9), (10), and (11), and the second affords the most convenient means of representing graphically the variations of the specific volume, since it is quite impracticable to plot the specific volume itself on an adequate scale.

Graphic Representation of the Variations of Specific Volume.

The best method of representing these results graphically appears to be that adopted in fig. 1, of plotting the ratio of v/V or $(c-b)/V$ against p. We have—

$$pv/R\theta = v/V = 1-(c-b)/V = 1-(c-b)p/R\theta \text{.........} \quad (27).$$

Since c is constant at constant temperature, the isothermals are straight lines inclined at different angles to the axis, but all intersecting at the same point, $v/V = 1$ when $p = 0$. These lines are drawn for each 20° of temperature in the figure, and are all represented as terminating in the saturation curve, although, as a matter of fact, it is

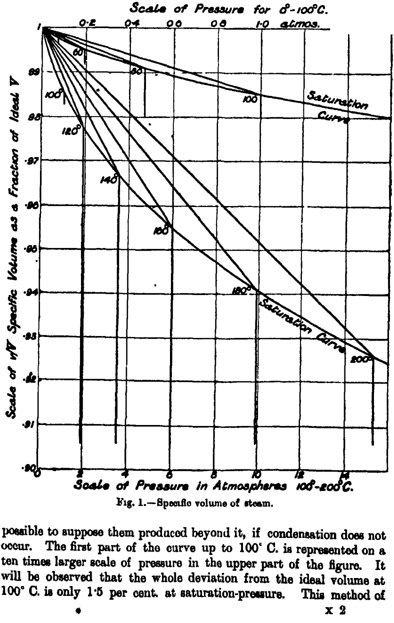

Fig. 1.—Specific volume of steam.

possible to suppose them produced beyond it, if condensation does not occur. The first part of the curve up to 100° C. is represented on a ten times larger scale of pressure in the upper part of the figure. It will be observed that the whole deviation from the ideal volume at 100° C. is only 1·5 per cent. at saturation-pressure. This method of plotting is analogous to that rendered familiar by Amagat and others in the case of the deviation of gases from Boyle's law. It is usual to plot the product pv against p, but it seems to me to be preferable to plot $pv/R\theta$ instead of pv, because the pv method confuses the diagram by introducing the effects of the variation of temperature, so that the different isothermals cannot be so well compared, and their relations observed.

In the diagrams of Amagat and others, who have adopted the direct method of measuring the whole specific volume instead of the differential method of observing only the deviation from the ideal volume, the isothermals are not accurately straight, but always bend downwards more steeply as saturation is approached, so that they are concave to the axis of pv. There seems reason to believe that this peculiarity may be partly due to the effect of surface condensation so well established by the observations of Ramsay and Young.* It is true that a similar though smaller increase in the slope results from the work of Natanson on the Joule-Thomson effect for CO_2, at pressures up to 26 atmospheres, at a temperature of 20° C. But in that case also the effect may be explained by condensation in the pores of the porous plug, as is indicated in some of the work of Joule and Thomson. I was not able to find by the differential method, which would eliminate any error of this kind, any trace of this effect at moderate pressures. In fact, the cooling effect appeared to diminish very slightly with increase of pressure, as it should, on account of the small increase in the value of the specific heat with increase of pressure. If this is generally true for other vapours, it would appear possible that many of the complications which have been introduced in current forms of empirical equations of the fluid state, may serve only to represent errors inherent in the experimental methods on which they are founded.

Values of the Specific Heats of Steam.

The values of the specific heats at any temperature and pressure are easily calculated from their limiting values at zero pressure, by means of the formulæ (9), (10), and (11) already given. The actual value of the specific heat at a pressure of one atmosphere was experimentally determined by the electrical method to be subsequently described. The value so found, though slightly larger than Regnault's, agreed so well with the theoretical value deduced from the characteristic equation (6) by means of Maxwell's assumption, that there can be little doubt that the method of deduction employed is valid. The value of the constant R for steam is readily found from the value of the ideal volume already assumed, we thus obtain†

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* 'Phil. Trans.,' A, 1892.

† Assuming that the pressure due to a column of mercury 760 mm. in height at 0° C. and sea-level in latitude 45° is equal to 1·0133 megadynes per sq. cm.

$$R = 4{\cdot}613 \times 10^6 \text{ C.G.S.} = 0{\cdot}4613 \text{ joule/deg. C.}$$
$$= 0{\cdot}11037 \text{ cal./deg. C.}$$

The unit of heat adopted in this paper is the thermal capacity of 1 gramme of water at 20° C., which is taken as being equivalent to 4·180 joules, from the mean of the results of Rowland and of Reynolds and Moorby, compared and reduced by the work of Callendar and Barnes on the variation of the specific heat of water over the whole range 0° to 100° C.

The limiting values of the specific heats of steam and of their ratio, in terms of this unit, are as follows:—

$S = 0{\cdot}4966$ cal./deg. C., $s = 0{\cdot}3862$ cal./deg. C., $\gamma = 9/7 = 1{\cdot}2857$.

In the following table the values of S and γ are given for saturated steam at the point of saturation, in order to illustrate the increase of specific heat with pressure. The values of the specific heat S at a pressure of one atmosphere at various temperatures are also given, to show the diminution of the specific heat with rise of temperature. The values enclosed in brackets are of course imaginary, but are included to show more clearly the nature of the change. The value of the specific heat at constant pressure has been calculated by Zeuner from Regnault's observations to be 0·368, on the assumption that the specific volume of steam is a linear function of the temperature at constant pressure, in which case the specific heat at constant pressure is independent of the pressure. Another common assumption is that the pressure at constant volume is a linear function of the temperature (Van der Waals). Neither of these assumptions can be reconciled with the most accurate thermometric work at moderate pressures or with the present experiments on steam by the method of the differential throttling calorimeter. The advantage gained by these assumptions is very slight and one-sided. The partial constancy of one specific heat is a small matter, if at the same time the other thermodynamical relations are rendered so complicated as to make the equations useless. The values of the specific heat of steam at constant pressure have been recently calculated by Grindley from his throttling experiments,* assuming the linear formula (1) of Regnault for the Total Heat of steam. These values are included in Table II for comparison. The numbers given in brackets are not given by Grindley, but are calculated by an extension of his method to show the effect of his hypothesis. The extraordinary differences between his values and mine, as shown in the adjacent columns (6) and (7) of Table II, are not due to any discrepancy in our experiments, but simply to his assumption of Regnault's formula for the total heat. The method of deducing the specific heat from the total heat, though it has often been applied, is unsound in principle, because the specific heat depends on the rate of variation of the total heat, so that all the errors in the formula for the total heat are enormously exaggerated in the calculation of the specific heat.

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* 'Phil. Trans.,' A, vol. 194, 1900, pp. 1–36.

The last column in the table contains the values of the so-called "Specific Heat of Saturated Steam," *i.e.*, the value of the specific heat when $dp/d\theta$ is determined by the condition that the steam is to remain saturated. The value in this case is given by the formula

$$S\,(\text{sat}) = dH/d\theta - L/\theta \quad . \quad . \quad (28),$$

which is most readily obtained by differentiating the expression for the entropy of saturated steam, namely, $\phi_s = \phi_w + L/\theta$, where ϕ_w is the entropy of water reckoned from 0° C. The values of S (sat.) also differ considerably from those which have been previously calculated on the usual assumption that $dH/d\theta$ is constant, and equal to 0·305, as in formula (1).

It will be observed that the mean value of the specific heat of steam at a constant pressure of one atmosphere between the limits 120° and 200° C., the range of Regnault's experiments, is 0·512 according to the values given in Table II. This value is not so greatly in excess of the value 0·48 given by Regnault as to be beyond the limits of experimental error, especially if we consider that the method which he adopted must necessarily have given rise to constant errors in defect, and that the superheat was only one-sixteenth of the total heat to be measured.

It is probable that the variation of the specific heats of all other gases and vapours, to which the Joule-Thomson equation can be applied, is of the same type as that exhibited above in the case of steam. I have succeeded in reconciling a good many of the apparently discordant experimental data on the subject by means of this hypothesis, but the experiments themselves are difficult, and the question of the variation of the specific heats of gases is obscured by unavoidable errors. Among the most remarkable and accurate of recent results are those of Joly on the specific heats of Air and CO_2 at constant volume, determined by means of his differential steam-calorimeter. The values which he obtained are much larger than those deduced from Regnault, and cannot be reconciled with them on the common assumption (Van der Waals) that the specific heat at constant volume is constant, but they agree remarkably well, considering the difficulty of the experiments, with the theory proposed in this paper. A fuller discussion of these and similar relations will be reserved for a future communication.

Table II.—Specific Heats of Steam

Temp Cent	At saturation point			Saturation pressure	At one atmosphere		S(sat.)
	Calories/deg		Ratio		Calories/deg		
t	S	s	q	p	S	Grindley	Cal./deg
				mm			
0	0.4973	0.3860	1.2882	4.66	(0.6060)	(0.306)	−1.680
20	0.4984	0.3873	1.2870	17.67	(0.5758)	(0.308)	−1.502
40	0.5008	0.3888	1.2888	55.55	(0.5544)	(0.314)	−1.351
60	0.5055	0.3919	1.2921	149.63	(0.5410)	(0.320)	−1.223
80	0.5128	0.3952	1.2977	355.30	(0.5318)	(0.349)	−1.116
100	0.5236	0.4009	1.3060	760.00	0.5236	0.387	−1.028
120	0.5388	0.4090	1.3175	1491.4	0.5181	0.448	−0.955
140	0.5581	0.4188	1.3327	2716.5	0.5138	0.537	−0.895
160	0.5816	0.4303	1.3518	4657.0	0.5105	0.665	−0.844
180	0.6086	0.4427	1.3748	7516.0	0.5073	(0.827)	−0.801
200	0.6393	0.4568	1.4012	11684.0	0.5050	(1.013)	−0.759

Variation of the Total Heat and Latent Heat

The Total Heat, H of a vapour defined in the usual manner, is related to the Latent Heat L, by the simple equation

$$H = L + h \qquad (29),$$

if the Heat of the Liquid h is reckoned from the same zero as the total heat of the vapour In the case of water both H and h are reckoned from the state of water at 0 C The specific heat of water is so nearly constant that h may often be taken as equal to t More generally we may write

$$h = t + dh \qquad (30),$$

where dh is the small difference of the heat h from the value t, which it would have if the specific heat were constant and equal to unity According to the observations of Barnes, the variation of the specific heat is very nearly linear between 60 and 100° C The value of h at temperatures above 60° C may be taken as

$$h = t + dh = t + 0.000110(t - 60)^2 \qquad (31)$$

This simple formula agrees very nearly with Regnault's observations in the rate of variation above 60°, and also with the table of values of h given by Callendar and Barnes.*

The accurate relation between the Total Heat and the Specific Heat of the Vapour is readily obtained by equating the intrinsic energy of steam evaporated at 0° C. at a pressure p_0, and then heated at constant pressure p up to any temperature θ to that of steam obtained by heating the liquid up to the same temperature θ, evaporating it at θ under the constant saturation pressure p, and expanding the vapour at constant temperature θ down to the pressure p_0 of saturation at 0° C. The various changes of intrinsic energy involved in these processes are given by equations (20) and (22). After a few simple reductions we obtain the Equation of Total Heat

$$H - H_0 = S(\theta - \theta_0) - (n+1)(cp - c_0p_0) \qquad (32),$$

which is simply the expression of the first law of thermodynamics as applied to the problem, and might also have been obtained in various other ways. If we omit the small terms depending on the co-aggregation c, the equation is identical with that given by Rankine in 1850, on the assumption that saturated steam could be treated as an ideal gas. The small terms represent the effect of the deviations of steam from the ideal state, and become important at high pressures. The equation neglects the external work of expansion of the liquid, but this is less than one-thirtieth of a calorie at 200° C., although it may become important as the critical temperature is approached.

Equation (32) gives only the variations of the total heat of the saturated vapour. In order to find the absolute values it is necessary to know the actual value of the total heat at some particular temperature. The obvious value to select would be that given by Regnault at 100° C., namely 637 calories. His methods do not appear, however, to have been sufficiently exact, and I prefer to rely on a more recent determination by Joly with his steam calorimeter (described by Griffiths).† Joly determined the mean specific heat of water between 12° and 100° C. in terms of the latent heat of steam at 100° C. Now the mean specific heat of water between 12° and 100° C. is known in terms of the specific heat at 20° C. by the results quoted above. We can therefore reverse the calculation, and find the latent heat of steam at 100° C. The result of the calculation gives L at 100° = 540·2 calories at 20° C. This is considerably in excess of Regnault's value, but it is quite within the limits of probable error of his experiments, and it possibly still errs in the direction of being too low. Assuming this value as a starting point, I have calculated the following table of values of the total heat and latent heat, and of the rate of variation of the saturation pressure and the total heat with temperature, namely $dp/d\theta$ (sat.) and $dH/d\theta$. It will be observed that the rate of increase of the total heat diminishes rapidly at high temperatures, while the rate of diminution of the latent heat increases. This must necessarily be the case, as the latent heat should vanish near the critical temperature, which occurs about 365° C. according to the observations of Cailletet and Colardeau, whereas the linear formula of Regnault would make the latent heat vanish at about 870° C. This is an additional indication of the impossibility of Regnault's formula. It may be observed, however, that the average rate of increase of the total heat, according to Table III. between 100° and 200° C. over the range of Regnault's experiments from which the linear formula (1) was calculated, is only 0.330 calorie per degree, which differs so little from the coefficient 0.305 given by Regnault as to be well within the limit of accuracy of his experiments, considering the acknowledged leakage of the distributing tap, and that the whole difference is only one half of 1 per cent. on the quantity of heat measured.

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* 'Brit. Assoc. Rep.,' 1899.

† 'Phil. Trans.,' A, 1895, p. 322.

The values of $dp/d\theta$ given in column 6 of the table are calculated from those of L by means of the thermodynamic relation (3) already quoted, assuming the values of the specific volume from Table I.

Table III.—Total Heat and Latent Heat of Steam in terms of the Thermal Capacity of Water at 20° C.

Temp. cent.	H−H° cals.	H cals.	h cals.	L cals.	$dp/d\theta$ mm./deg.	$dH/d\theta$ cals./deg.	Regnault's formula
0	0.00	593.5	0.00	593.5	0.3301	0.4935	606.5
20	9.83	603.3	20.06	583.2	1.09	0.4887	612.6
40	19.53	613.0	40.02	573.0	2.952	0.4800	618.7
60	28.90	622.5	60.00	562.5	6.901	0.4665	624.8
80	38.15	631.7	80.03	551.7	14.35	0.4467	630.9
100	46.83	640.3	100.14	540.2	27.104	0.4202	637.0
120	54.90	648.4	120.3	528.1	47.45	0.3885	643.1
140	62.33	655.8	140.6	515.2	77.06	0.353	649.2
160	68.9	662.4	161.1	501.3	118.62	0.314	655.3
180	74.9	668.4	181.6	486.8	173.50	0.274	661.4
200	79.9	673.4	202.3	471.1	243.0	0.237	667.5

The values of $dH/d\theta$ are obtained from those of $dp/d\theta$ by differentiating equation (32), thus,

$$dH/d\theta = S' - (n+1)c(dp/d\theta - np/\theta).$$

It will be observed that the values of the total heat in the above table agree very closely with those of Regnault between 60° and 90°, where his results (according to Griffiths)* are most reliable. The average of his observations over this range is in exact agreement with Table III. At lower temperatures, Regnault's observations are very discordant, but the values given in the table are well supported by those of Griffiths. He finds, for instance, the latent heat to be 572·7 calories at 40·2° C., where the table would give 573·1. The unit employed by Griffiths (calorie at 15° C.) is different, so that his value would require to be raised nearly 0·6 cal. to reduce to the same unit, which would make it 573·3 calories at 40·2°. There can be no doubt that his observations are entitled to much greater weight than those of Regnault, which are nearly 6 calories larger at this point. The value, 596·7 found by Dieterici with an ice calorimeter,† for the latent heat at 0° C., is 3·2 calories larger than that given in the table. But it must be remembered that observations of the latent heat at 0° C. are not at all easy, and that there is some uncertainty about the unit employed by Dieterici, as he finds by the same method the value of the mean specific heat of water between 0° and 100° C., about 1 per cent. larger than Reynolds and Moorby or Callendar and Barnes. It is possible that his result might agree with Table III if it could be reduced to the same units.

Entropy of Water and Steam.

The entropy of water, ϕ_w, is readily calculated from a table of the values of the specific heat when the variation of the specific heat is known. Since, however, the specific heat is nearly equal to unity, we may write,

$$\phi_w = \log_e \theta/\theta' + d\phi \quad \ldots\ldots\ldots \quad (33),$$

where $d\phi$ is the small difference of the value at any temperature from the value $\log_e \theta/\theta°$, calculated on the assumption of constant specific heat. The values of $d\phi$ given in the following table have been calculated from the table of values of the specific heat given by Callendar and Barnes.‡ It will be seen that the values of $d\phi$ are very small at temperatures below 100° C., but increase rapidly at higher temperatures. Above 200° C. the values of the specific heat of water are so uncertain that it is not possible to calculate the properties of steam satisfactorily by any method. The values of $dh/\theta = (h-t)/\theta$ are also given in the table for comparison with those of $d\phi$. They are calculated from the same table of the values of the specific heat, and the difference $d\phi - dh/\theta$ is seen to be small. This difference occurs as a small correction in the equation for the saturation pressure, to be presently given. The values of the entropy of water, corrected for the variation of the specific heat are given in the column headed ϕ_w. The values of the entropy of steam are obtained by adding the values of L/θ given in the next column, which are found from the values of the latent heat already given in Table III. Values of ϕ_s, calculated from Regnault's formulæ, are given in the last column for comparison.

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* 'Roy. Soc. Proc.,' December, 1894.

† 'Wied. Ann.,' vol. 37, p. 504, 1899.

‡ 'Brit. Assoc. Rep.,' 1899.

Table IV.—Entropy of Water and Steam.

t	dh/θ.	$d\phi$	$\log_e \theta/\theta^{\circ}$	ϕ_w.	L/θ.	ϕ_s.	Regnault
0°	0	0	0	0	2·1740	2·1740	2·214
20	0·00016	0·00017	0·07070	0·07087	1·9908	2·0617	2·080
40	0·00008	0·00009	0·13672	0·13681	1·8308	1·9676	1·982
60	0·00000	0·00001	0·19867	0·19868	1·6892	1·8880	1·890
80	0·00009	0·00010	0·25694	0·25704	1·5628	1·8198	1·814
100	0·00037	0·00039	0·31208	0·31216	1·4483	1·7608	1·748
120	0·00084	0·00090	0·36428	0·36518	1·3438	1·7090	1·692
140	0·00152	0·00163	0·41104	0·41567	1·2475	1·6632	1·644
160	0·00242	0·00261	0·46112	0·46373	1·1578	1·6215	1·604
180	0·00353	0·00384	0·50645	0·51029	1·0746	1·5849	1·568
200	0·00497	0·00532	0·54960	0·55192	0·9960	1·5509	1·536

The Entropy Equation

By the application of the second law of thermodynamics we may obtain a relation between the latent heat and the saturation-pressure. Expressing the fact that the entropy of steam evaporated at θ°, and heated at constant pressure p° up to any temperature θ, is the same as the entropy of steam obtained by heating water up to the same temperature, θ, evaporating it at saturation-pressure, p, and then expanding it at constant temperature, θ, down to the pressure, p°, of saturation at θ°, we immediately obtain the entropy equation

$$(1 - S^{\circ}) \log_e \theta/\theta^{\circ} + L/\theta - L^{\circ}/\theta^{\circ} + R \log_e p/p^{\circ}$$
$$+ d\phi_w + ncp/\theta - nc^{\circ}p^{\circ}/\theta^{\circ} = 0 \ldots\ldots \quad (34).$$

The various terms of this equation have been already calculated, and are given in equations (18), (24), (26), and in Table IV. The last three terms of the equation are small, and represent the effect of the variation of the specific heat of water, and of the deviation of the properties of steam from the ideal state, as expressed by the characteristic equation (6). Neglecting the small terms, the equation is identical with one given by Bertrand, on the assumption that steam may be treated as an ideal gas. It is evident that the values of the saturation-pressure may be calculated from this equation by means of the values of the latent heat already found. It is better, however, to eliminate L by means of the energy equation (32) already given.

Equation of the Saturation-Pressure.

If we substitute $H - H^\circ = L - L^\circ + h = L - L^\circ + t + dh$, in the energy equation (32), and divide by θ, and subtract from the entropy equation (34) above given, we obtain the equation of saturation-pressure, which may be reduced to the form

$$R \log_e p/p^\circ = (L^\circ/\theta^\circ + n p^\circ c^\circ/\theta^\circ) t/\theta - (1 - S^\circ)(\log_e \theta/\theta^\circ - t/\theta)$$
$$+ (pc - p^\circ c^\circ)/\theta - (d\phi - dh/\theta). \;\ldots.\; (35).$$

Neglecting the terms depending on the co-aggregation, and on the variation of the specific heat of water, this equation is equivalent to one given by Dupré and Bertrand, and rediscovered in various ways by many other observers (*e.g.*, Pictet and Hertz).* If the correct values of S°, L°, and R are inserted in the formula, the equation thus simplified gives very accurate values of the saturation-pressure at low pressures where the properties of steam satisfy approximately the fundamental assumptions made in deducing the formula. Bertrand,† although of course he was well aware that the formula thus obtained was not accurate at high pressures, has calculated numerical formulæ of this type for a large number of vapours, choosing the constants empirically so as to obtain the best agreement over the whole range. The values of the constants so found do not, of course, agree with the correct values of L° or S°. The numerical values chosen by Bertrand in the case of water, for instance, give L° = 573 calories, S° = 0·575 cal. per deg., and the value of the steam pressure found is 763 mm. at 100° C. At low temperatures the first term in formula (35) is the most important, since $\log_e \theta/\theta^\circ$ is very nearly equal to t/θ when t is small. The formula then reduces to the simple type, $\log p = A + B/\theta$, which has often been employed for approximate work, and is the basis of the useful relation of Ramsay and Young.‡ Adding a second term, C/θ^2, to this formula to take account of the small terms in equation (35), we obtain the well-known empirical formula of Rankine (1849), which is very convenient and accurate. A nearly equivalent formula is that of Unwin,* $\log p = A + B\theta^{p}$, in which the same effect is empirically secured by an arbitrary exponent. These formulæ are purely empirical, but it is interesting to observe how they are related to the correct thermodynamical expression (35).

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* Pictet, 'Comptes Rendus,' vol. 90, p. 1070, 1880 (proof invalid); Hertz, 'Wied. Ann.,' vol. 17, p. 177, 1882.

† 'Thermodynamique,' p. 93.

‡ 'Phil. Mag.,' vol. 21, p. 33.

Saturation Pressures of Steam

In employing equation (35) to calculate the numerical values of the saturation-pressure in the case of steam, we have only one empirical constant, namely, p°, which is determined by the condition that the saturation-pressure at 100° C. is 760 mm. The values of the other constants which occur in this equation have been already given, namely,

$$S^\circ = 0{\cdot}4966 \text{ cal/deg} \qquad L^\circ = 593{\cdot}5 \text{ cals} \qquad R = 0{\cdot}11037 \text{ cal/deg.}$$

The value of θ°, which is also one of the fundamental data, is taken as being 273°, but is uncertain to the extent of 0·1°. Dividing the equation by R, and reducing to common logarithms by the modulus $\log_e 10 = 2{\cdot}3026 = m$, we obtain the numerical formula

$$\log_{10} p/p^\circ = 8{\cdot}554\,t/\theta - 4{\cdot}561\,F(\theta) + (pc - p^\circ c^\circ)/mR\theta \\ - (d\phi - dh/\theta)/mR \ldots\ldots \quad (36).$$

in which $F(\theta)$ stands for the function $\log_{10} \theta/\theta^\circ - t/m\theta$. I have used this form for calculation, and have given the values of the separate terms in Table V so as to show their relative importance. It is also possible to write the formula in the shape, $\log p = A + B/\theta + C \log \theta$ + small terms, but this does not show so clearly the relative effect and importance of L° and S°.

The values of the saturation-pressure in the column headed p are calculated by the complete thermodynamic formula (36). The values given in the column headed Regnault are those of Regnault, recalculated by Peabody and reduced to latitude 45°. The difference expressed in degrees of temperature is given in the last column, and is probably within the limits of error of Regnault's observations and of the empirical formulæ employed to represent them. If we refer to the actual observations of Regnault, we find that the discrepancies of individual observations at any point, expressed in degrees of temperature, exceed the values of the differences shown in the last column. We also find that in most cases the actual observations agree better with the single formula (36) than they do with the two empirical formulæ, each with five arbitrary constants, from which the values in the column headed

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* 'Phil. Mag.,' vol. 21, p. 300.

Table V.—Saturation-Pressures of Steam.

t.	$8{\cdot}554\,t/\theta$	$-4{\cdot}861\,F(\theta)$	$+\frac{(pv-p'v')}{aR\theta}$	$-\frac{d\phi-dh\,\theta}{aR}$	p	Regnault	Diff.
°0	0	0	0	0	4·66	4·60	0·18
20	0·5838	−0·0019	+0·0003	0	17·68	17·40	0·26
40	1·0932	−0·0177	+0·0009	0	53·55	54·91	0·20
60	1·5413	−0·0367	+0·0020	0	149·63	148·80	0·12
80	1·9387	−0·0603	+0·0039	0	355·30	354·63	0·04
100	2·2932	−0·0872	+0·0066	−0·0001	760·00	760·00	0·00
120	2·6120	−0·1169	+0·0104	−0·0002	1491·4	1490·5	0·02
140	2·8998	0·1188	+0·0152	−0·0005	2716·5	2717·9	0·02
160	3·1612	−0·1817	+0·0211	−0·0008	4657	4652	0·05
180	3·3990	−0·2163	+0·0279	−0·0012	7546	7537	0·06
200	3·6170	−0·2514	+0·0357	−0·0018	11684	11664	0·08

Regnault were calculated. Taken in conjunction with the differential throttling experiments, and with the direct measurement of the specific heat by the electrical method, this is very strong evidence that Regnault's formula for the total heat is incorrect, and that the values of the total heat and latent heat given in Table III, and supported by the experiments of Griffiths and Joly, should be accepted in its place.

"Note on Inquiries as to the Escape of Gases from Atmospheres." By G. JOHNSTONE STONEY, M.A., Hon. D.Sc., F.R.S. Received and Read June 21, 1900.

We have now three investigations which profess to supply information about the escape of gases from atmospheres. Two of them, those of Messrs. Cook and Bryan, reason forward by the help of the kinetic theory of gas from the supposed causes; the third, which is that preferred by the present writer, reasons backward by the help of the same theory from the observed effects.

Mr. Cook's investigation, which will be found in the 'Astrophysical Journal' for January, 1900, seeks to compute the proportion of molecules which can attain the speed requisite for escape by means of the formula which Maxwell published in 1860, assigning the proportion of particles whose speed lies between v and $v+dv$, in a system of colliding particles intended to represent an isotropic portion of gas.

Professor Bryan's investigation* is based on the investigations made since 1866 into the way in which energy tends ultimately to be partitioned among the various motions possible within a self contained dynamical system of bodies. The system need not be isotropic, since the bodies may be moving in a constant field of force.

An inquiry by the present writer into Mr Cook's method of dealing with the problem is attempted in the May and June numbers of the 'Astrophysical Journal' for 1900, and in the present paper a similar attempt is made with reference to Professor Bryan's.

Both Mr Cook and Professor Bryan predict the proportion of molecules which can escape from an atmosphere by deducing the proportion from its supposed causes, and in this respect are in contrast with an investigation previously published, which sought to ascertain from the observed effects of escape where and on what scale it has in fact taken place. (See memoir by the present writer in the 'Scientific Transactions of the Royal Dublin Society,' vol 6, Part 13, or in the 'Astrophysical Journal' for January, 1898. And for further evidence that helium is escaping from the earth, see 'Nature' of the 24th May, 1900, p 78.)

Where, as in the present instance, the *a priori* and *a posteriori* methods have led to inconsistent numerical results, there must be a mistake or mistakes somewhere, and it is incumbent upon us to search till these are detected. If they can be found and corrected an important advantage will be gained. Professor Bryan, at the end of his letter in 'Nature' of the 7th June, 1900, indicated one place where a mistake may have been made, viz, in the assumed relation between temperature and the kinetic energy of the translational motions. Another mistake may perhaps have been made in assuming the legitimacy of treating the partition of energy when molecules move in a field of force, as though the only partition to be considered is between these molecules, whereas no field of force can exist unless it has been produced by some physical agent, upon which every motion that goes on within the field must react. In consequence of these reactions no field of force in which any motion occurs can be accurately constant, and a partition of energy based upon the supposition of its constancy is a theorem in rational dynamics, but has no counterpart in nature.

Thus, in the case of the earth's atmosphere, the anisotropic condition of its outer layers is due to the field of force which exists in the neighbourhood of the earth; and when we are obliged to take into account this anisotropic condition, *as we must when dealing with the escape of gases from atmospheres*, this is to be done (when we are treating the problem as one of partition of energy) by including as molecules between which the partition has to take effect not only the gaseous molecules, but also all the other attracting molecules which provide the field of force.

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* 'Roy. Soc. Proc.,' April, 5, 1900, p. 383.

[So again with reference to the never-ceasing turmoil which goes on in the atmosphere, which near the surface of the earth exhibits itself in tempests, thunderstorms, and other phases of weather, and which in the upper regions includes phenomena still more extensive and swift. It is manifest that these events increase the opportunities which gaseous molecules have of escaping from the earth, and that accordingly *they must be taken into account*, either explicitly or implicitly, in every valid inquiry as to the rate of escape.

To take them into account in an investigation based on the partition of energy, we have to extend that partition to whatever agency produces the turmoil. Now the activity within the atmosphere (and in fact almost every molar activity upon the earth other than the little which is attributable to tidal action or to such minor agencies as earthquakes and volcanoes) is caused by the shiftings about of energy which come in between the continuous advent of energy by radiation from the sun, and its continuous escape from the earth by radiation into space. Hence to render an investigation by the Boltzmann-Maxwell law valid it is necessary to extend the partition of energy beyond the atmosphere—first to the solid earth, so as thereby to take account of the anisotropic character of some of the atmospheric strata (which facilitates the escape of gas); and secondly to embrace at least the sun and the æther between the earth and sun, so as thereby to take into account the turmoil in the upper regions of the atmosphere (which further increases the rate of escape). It seems to be only when these extensions shall have been effected that a generalised law such as the Boltzmann-Maxwell law for the partition of energy between the various degrees of freedom can become competent to furnish any information with reference to the rate at which gaseous molecules actually do escape from the earth.—July 17, 1900.]

Then as regards temperature. The temperature of a solid is in reality twofold; it is either its radiation temperature or its conduction temperature. These are physically distinct, although in all but some exceptional cases they are so nearly proportional to one another that they may be given the same mathematical expression. So, again, when dealing with gases we do well to keep in mind the essential distinction between radiation temperature and what may be called convection temperature. The temperature of an isolated gaseous molecule moving by itself through space is of the first kind only, and depends exclusively on the energy of the internal motions—those motions within the molecule which enable it to absorb or emit radiant heat—and *it is in no degree affected by the kinetic energy* of the translational motion of the molecule; whereas if the same molecule form part of a gas, it meets with encounters with other molecules or with the walls of a containing vessel, and at each such encounter there is a partition of energy between the translational and the internal motions, and in consequence of this the kinetic energy of the translational motion becomes a part of what determines that average power of absorbing and emitting radiant heat which (when estimated over a time embracing a sufficient number of encounters) is the proper definition of the radiation temperature of the molecule. Accordingly the average kinetic energy of the translational motions of the molecule enters into its mathematical expression. If the gas be dense, encounters are frequent, and Δt, the time requisite for the averages, may be brief. In this case the radiation temperature of a molecule, while the gas is undergoing some change in its condition, is predominantly the outcome of its encounters, and depends mainly on the molecules that surround it; whereas if the gas be very much attenuated, then the radiation temperature of the molecule during a period of transition will depend mainly on what influences then reach it from the surrounding æther, and will be but in a subordinate degree affected by the encounters to which the molecule at about that time happens to be subjected

This is a matter which needs to be very fully taken into account when we attempt to estimate the escape of molecules from the earth's atmosphere, inasmuch as a large part of the heat radiated by the sun to the earth is absorbed by the gaseous molecules which happen at the time to be moving about in those strata of the atmosphere from which alone there can be any effective escape Accordingly it will need to be carefully scrutinised whether this has been either explicitly or implicitly taken into account in the attempts which have been made to determine *a priori* the rate of escape.

When the molecules of a gas or of a mixture of gases move in a field of force such as that surrounding the earth, convection currents can exist, and the term temperature as applied to the gas becomes ambiguous. It may have either of two distinct meanings, one of which has reference to the transport of heat by convection and by the consequent sweeping of successive portions of gas against bodies immersed in it, and the other has reference to the exchanges of heat by radiation with those or with more distant bodies. These are different physical events, and the assumption that they stand in a fixed ratio to one another is convenient, but is often not true. It is probably legitimate to regard it as approximately holding good in a gas which has nearly reached a final, *i.e.*, an unchanging condition, and where the problem with which we are dealing does not need our making any closer scrutiny than as to what on the average happens to a sufficiently large swarm of molecules within a sufficiently long duration; but it is not true while gas is passing through transition stages, nor is it true of individual molecular events or of small swarms of events, even in gas which has reached its final state.

Now none of the gases of the atmosphere have even approached any such state. Changes incessantly go on in the open air at the bottom of the atmosphere and the extent and abruptness of the changes that as incessantly go on in its upper regions are probably greater.

Again the consequences of cumulative effects arising in the illimitable trains and combinations of encounters that are taking place and of associated events in the aether will also need to be either explicitly or implicitly taken into account in any valid investigation of the escape of gases from atmospheres by the deductive method.

All the circumstances that have been referred to would have to appear among the data of an ordinary dynamical investigation of the escape of an individual molecule from an atmosphere if such an investigation were possible and the claim of a generalised theorem like that of the partition of energy to render it unnecessary to go into these details ought to be carefully scrutinised. In one case at least the claim does not appear to stand this test viz in reference to the supposed legitimacy of the assumption that the field of force surrounding the earth is constant. Though its variations are minute they are none the less real and are due to interactions between each gaseous molecule and all the molecules of the solid earth as real as are the interactions between gaseous molecules when they encounter and as much entitled to be taken into account when we seek to carry on the investigation in the region of generalised propositions. It should be kept in mind that in reference to what happens within this region, the plea of being so minute as to be of negligible amount is not admissible. Whether a very small factor may or may not be neglected must be determined independently in each individual case and in the above instance the decision is that it may not be neglected.

Other corrections might be suggested along with the principal ones noticed above—that relating to the two kinds of temperature, that relating to the field of force and that relating to turmoil in the atmosphere but what seems most to be wanted is that we should recognise that any law for a distribution of energy within the atmosphere by itself can only come approximately into practical effect after the lapse of a sufficient duration and throughout a column of the atmosphere from which accidents are excluded, and that this law will not be the Boltzmann Maxwell law, which may not be so restricted.

Thus let us imagine a cylinder like a great Tower of Babel, reaching to the top of the atmosphere, with walls competent to intercept dynamical, electrical and all other extraneous influences other than gravitation. The air within this tube would consist of molecules, moving in a field of force caused mainly by the earth's attraction and rotation, and this column of air might perhaps after some such period as a month, a year, century, or a thousand years nearly attain such a distribution of energy as that indicated by some law. But if while this process is maturing a wind overthrows the tower, sweeping away the air it contained and substituting other air under new conditions, and subject to all the chances of uprushes, downrushes, thunderstorms, auroras, cyclones, cloud, sunshine, rain, &c., then after all or any of these or of the like accidents, the tower would have to be rebuilt before any portion of the atmosphere extending from the bottom to the top could find itself in a position even to commence the first steps of an advance towards at some future time complying with the law.

The supposition then that the Boltzmann-Maxwell law can be restricted within our existing atmosphere would appear to be a mistake; and if so the inferences from that law are not part of a real interpretation of nature. It need not therefore be matter of surprise that, in the case of helium, the facts of nature seem to negative those inferences.

The weather which will prevail over the earth this day month will be the outcome of the present molecular state of the earth, and of the molecular events which will happen in the meantime; but our power of stating in mathematical form the existing state of the earth, and our knowledge of molecular physics, are not such as would enable us to predict that future weather by the *a priori* or deductive method of proof. The difficulties in this case are easily seen; and they are instructive, since the escape of gas from the earth depends on phenomena which are probably as complex as those which determine the weather and as little amenable to treatment by the deductive method.

Any such distribution of energy as that assigned by the Boltzmann-Maxwell law would, if it could be realised, be brought into existence by the gradual effacement of excesses which had previously existed; from which it would appear to follow that excesses prevail in our existing atmosphere greater and more numerous than could exist in an ideal atmosphere that obeyed that law. It is probable, therefore, that in our actual atmosphere there are more opportunities for the escape of molecules than there would be in the ideal atmosphere—a conclusion which accords well with the fact that the actual rate of escape exceeds those computed by Professor Bryan and Mr. Cook. (See 'Nature' of May 24, 1900, p. 78, second column.)

"South African Horse-sickness: its Pathology and Methods of Protective Inoculation" By ALEXANDER EDINGTON, M.B., C.M., F.R.S.E., Director of the Colonial Bacteriological Institute, Cape Colony. Communicated by Sir DAVID GILL, F.R.S.

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This disease, so far as is known, is peculiar to South Africa.

While affecting the Transvaal and Rhodesia every year—beginning about the end of October and continuing until the following May, or even later—it only affects the Cape Colony and Natal in an epizootic form in certain years, although sporadic cases occur annually in certain localities.

Animals Affected.—It affects horses, asses, mules, and it has been asserted—although I have never seen such cases—that quaggas have also been killed by it.

A disease which occurs to a limited extent among cattle, called by the natives Imapunga, and one which exists as a widespread plague among high-bred sheep and goats in the Eastern Province of Cape Colony, are each closely related in their pathology with this malady.

Areas Infested.—The most dangerous areas are those which are relatively low-lying—independent of the absolute altitude of the district.

Period of Infection.—It has been commonly observed that where animals during a season of sickness are not permitted to graze after sunset and before the sun has dried up the dew from the herbage, they do not so commonly become affected as where such a routine is not carried out.

Horses which are stabled during the night are, as a rule, safe, but during last year 60 per cent. of the stabled horses in Eshowe, Zululand, died of this sickness. Veterinary Lieutenant Coley, A.V.D., who kindly made the observations for me, stated that these horses were mainly fed on Guinea or Ubaaba grass mixed with forage or Indian corn. This grass was usually cut in the evenings and made into bundles till next day. Those who took particular care to have the grass thoroughly dried in the sun before using it did not lose their horses, while those neglecting this precaution lost heavily.

The disease is only directly contagious; for while inoculated horses have died in my stables among clean animals, I have never found, during observations extending over seven years, a single case of infection from such a source.

The annual mortality in Rhodesia and the low-lying parts of the Transvaal amounts to over 90 per cent.

Animals which have recovered from the sickness are termed 'salted," and are from six to ten times increased in market value.

Secondary Fever

Animals which are "salted" are liable to subsequent attacks of fever which have no necessary relation to fresh infection. I have observed numerous cases of this description among the "salted" animals under my observation and during periods when the sickness was unknown.

Symptoms of the Disease

It occurs under two forms the Dikkopziekte* and the Dunpaardeziekte † In the former the head and neck swells up enormously, thus affording trustworthy indications of illness during life. In the latter form, as a rule, no symptoms appear until close to the period of death, when the animal becomes subject to very rapid breathing with heaving at the flanks. At the moment of death, in both forms, it is common to find a huge cloud of white foam ejected from the mouth and nose. This foam is produced from a free exudation of blood plasma into the air passages

Owing to the fact that the animals suffering from the Dunpaardeziekte show no symptoms until toward the end of the period of illness, it had come to be believed that the whole period of the disease was limited to a few hours' duration.

Post-mortem Phenomena

The pericardium is almost invariably filled with a yellow fluid which, while usually clear, is sometimes blood-stained. Solidified gelatinous exudate is frequently found in relation to the beginning of the aorta. The pleural cavity is frequently occupied by yellow fluid, and the interlobular and sub-pleural tissues are also frequently distended by this material. The interlobular tissue is frequently so distended by exudation that the lung tissue proper is dissected up in all directions. The subcutaneous tissue, especially about the great vessels in the neck, is commonly found to be invaded by this exudation, while in the Dikkopziekte the swelling of the head and neck is due to this effusion.

The yellow fluid of the pericardium and the pleuræ is spontaneously coagulable in the presence of minute traces of blood.

These represent the more characteristic pathological conditions obtaining in this disease, among which one characteristic is most noticeable by its absence, *e.g.*, inflammatory phenomena. Pathological phenomena are, therefore, for the most part to be ascribed to the marvellous exudation of blood plasma, which, while seen more or less throughout the serous and subcutaneous tissues, is best marked within the thoracic cavity.

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* **Dikkopziekte**, a Dutch word signifying "thick-head sickness," is applied to the form in which the swollen head is the most obvious symptom.

† **Dunpaardeziekte**, "thin horse-sickness," applied to the form in which the head is little or not at all swollen.

In my annual reports as Director of the Colonial Bacteriological Institute I have referred to the morbid anatomy in greater detail.

Inoculation Experiments.

For the purpose of conveying to healthy animals the infection from those already sick three materials have been made use of, viz.—

1. The yellow fluid from the trachea of infected animals.
2. The yellow fluid from the pericardium of infected animals.
3. The blood of infected animals.

The use of the first two fluids has not always been successful in setting up the disease, but fresh virulent blood has invariably proved successful.

Methods of Use of the Materials mentioned.

(*a*.) By subcutaneous injection.

(*b*.) By insertion of a seton, impregnated with the fluid, under the skin.

(*c*.) By drenching, *e.g.*, giving a dose by the mouth.

Sites Selected.

(*a*.) Directly into the lung tissue by hypodermic needle operating through the skin over an intercostal space.

(*b*.) Into the subcutaneous tissue of the neck.

(*c*.) Into the subcutaneous tissue of the flank.

(*d*.) Intravenously.

Any one of the channels selected is equally suitable, but the incubation period is somewhat shorter when the intravenous method is used.

Period of Incubation.

When the malady is induced by the inoculation of 2 or 3 c.c. of the blood of an animal which has died from spontaneous disease, a mean period of eight to nine days supervenes, after which the temperature begins to rise. The elevation is gradual, with remissions during the night, but attains to 106° F., as a rule, before death, which usually occurs after four or five days of fever.

Preservation of the Virus.

After having transmitted the disease through a succession of animals, I found it possible to preserve its virulence unimpaired through a long period of time.

I bleed the animals into bottles which hold 800 c c. of fluid These bottles are prepared by placing in them 50 c.c. of a 10 per cent. solution of neutral citrate of potash and plugging the necks with cotton wool. Such bottles are sterilised in the autoclave previous to use. After being filled with blood, the influence of the citrate arrests coagulation, and the corpuscular matter soon subsides, leaving the more or less clear plasma above The latter is drawn off to half the original bulk, and is replaced by a 50 per cent. solution of glycerine and water, containing 0 25 per cent. of pure carbolic acid. Such a mixture preserves its virulence quite unimpaired for over two years One c.c. of this material serves to induce the disease in its characteristic form, but if the dose is increased to 3 or 5 c.c, the period of incubation is shortened, and the post-mortem phenomena are less characteristic.

From the observations I have made I have found that the subcutaneous injection of fresh or properly preserved horse-sickness blood produces symptoms during life, and shows pathological changes after death which are not to be distinguished from those found in the spontaneously occurring cases of the lung form of the disease. It is somewhat remarkable that the *only* cases in which I have succeeded in producing the Dikkop form were those in which the virus which was used was somewhat septic. When, however, I have inoculated virulent preserved blood into partially protected animals, I have in a number of cases, although not in all, produced this form. In three cases where the virus used has been sufficiently attenuated as not to produce death but a longer febrile period than is found in the fatal cases, I have also seen the Dikkop form produced.

Effect of Desiccation on the Virus.

Citrated blood dried in a thick layer was rendered non-virulent. Where, however, such blood was rapidly dried on glass plates in very thin layers it was found, when 2 grammes was dissolved in salt solution and injected into horses, that it produced fever, but not in a virulent form. The fever thus induced gave practically no protection against a dose of virulent blood at a subsequent date. Mild attacks of horse-sickness do not, however, give such protection as is required to resist virulent blood.

Experiments made in regard to Protective Inoculation.

The yellow fluid from the pericardium of an animal which had died from the disease was filtered through a Pasteur filter. 100 c.c. of the filtrate was injected subcutaneously into a horse. Eleven days later it was inoculated with 3 c.c. of preserved blood injected subcutaneously. The result, which culminated in death from ordinary horse-sickness, showed that no protective influence had been exerted by the filtered fluid.

Effect of Calomel.—This drug, in doses of 30 to 60 grains daily, had the effect of retarding death, and the blood of such animals drawn at periods later than that at which death usually occurs was distinctly weakened in virulence. Such blood has on several occasions, though not in all, induced attacks of the disease, from which the animals not only recovered, but acquired protection against virulent blood injected subsequently.

Transference of the Disease to other Animals.

(*a.*) *Donkeys.*—The subcutaneous or intravenous inoculation of donkeys with fresh virulent blood is followed by fever. The period of onset is irregular and uncertain, while the duration of the febrile period varies from one or two days to, in my experience, a week or more.

The amount of the virus used has some relation to the severity of the fever, but the special susceptibility of the animal is the principal factor in determining the degree and duration of the fever.

Two donkeys, equal in age, were inoculated respectively with 1 c.c. and 50 c.c. of the same blood. In both cases a moderately severe reaction followed, and while the animal receiving the injection of 50 c.c. was rather more severely affected than the other, the difference on the whole was but slight.

In all I have inoculated twelve donkeys, and, while none died, the difference in susceptibility was most clearly demonstrated, some scarcely showing any reaction at all.

(*b.*) *Cattle.*—The susceptibility of cattle to the inoculated disease is excessively variable. I have inoculated twenty-one cattle. A definite febrile reaction was produced in seven cases, and four died.

In the case of one which died, and in which the symptoms produced were quite characteristic of those found in horses, the inoculation of its blood into a clean horse was followed by the usual period of incubation, the onset of fever, and death from characteristic horse-sickness.

The disease known as Imapunga, which occurs to a limited extent among cattle, presents features which in every respect are identical with those produced in susceptible cattle by the inoculation of virulent horse-sickness blood.

(*c.*) *Goats.*—Angora goats are also to a limited extent susceptible to horse-sickness infection. Of seventeen which were inoculated, a febrile reaction occurred in ten; none died. From one of the ten blood was taken, which was used to inoculate an ox. The latter animal developed fever, and died with exactly similar symptoms during life, and showed the same post-mortem conditions as the ox already referred to, whose blood, when inoculated into a horse, produced characteristic horse-sickness.

(*d.*) *Sheep.*—Sheep are also susceptible Of ten which were inoculated, six showed a well marked febrile reaction, but none died.

I have not succeeded in transferring the disease to rabbits, guinea-pigs, rats, or mice.

The Transmission of the Disease for Protective Inoculation by Means of the Inoculation of Fresh Blood

The inoculation of horses with the blood of donkeys which were suffering from fever produced by inoculation has been attended with most varying results.

In some cases death has been produced, in some an irregular febrile period, while in others no apparent result has followed The period of onset of the fever has likewise been most variable In some cases a reaction has been set up corresponding to the normal period of incubation which obtains in horse-sickness, while in other cases reaction has been delayed for more than 25 days.

Influence of the Reaction produced

Where fever has set in on or about the eighth day, been moderately severe in degree and duration, and subsequently subsided, a very definite degree of protection has been produced, although seldom high enough to set up such a resistance as to oppose death when the animal was subsequently inoculated with virulent blood.

A striking demonstration of variable susceptibility among horses was furnished during these experiments. Of three horses and one mule which were each inoculated subcutaneously with 5 c.c. of fresh blood

The mule had no reaction.
Two horses had scarcely any reaction.
One horse had a good reaction.

In the case of the last horse, when subsequently inoculated with virulent blood it suffered severely and just managed to recover. The others had not been protected to any appreciable degree. Obviously, therefore, the susceptibility of the last animal had been such as to admit of infection from the donkey's blood producing reaction and the establishment of protection, whereas the higher degree of insusceptibility of the other animals resisted infection, and in this way evaded the onset of protection. This phenomenon forms the greatest barrier to protective inoculation, and has contributed to the enormous trouble I have experienced in devising a practical method of protective inoculation.

The fresh infected blood of cattle, sheep, and goats is still more variable in its results than that obtained from the donkey.

Numerous other experiments of the same nature have been made, all of which result in showing

(*a*.) That donkeys, oxen, goats, and sheep possess a very irregular susceptibility to the disease.

(*b*.) That the blood of donkeys which do not react may produce no effect when inoculated into the horse.

(*c*.) That the blood of donkeys which have evinced moderate reaction may produce intense reaction in some horses and practically none in others

(*d*.) That a mild reaction in the donkey furnishes no definite assurance as regards the reaction which its blood may set up in horses.

Owing to the variable quality of the infection possessed by infected donkey's blood in the fresh state, I experimented with blood taken from donkeys and oxen which, after having been received, was preserved in the manner already described.

A large number of experiments carried out by this means furnished the following results:—

1. Protection was only obtained where a definite amount of fever had been produced on several occasions, but unless the reaction was severe, the animal did not resist the inoculation of 1 c.c. of preserved virulent blood at a later period.
2. The susceptibility of horses to such a weakened or attenuated virus varies enormously. Of two animals inoculated with the same dose of the same virus injected directly into the jugular vein, one had good reaction, the other very feeble. Neither were found to be protected when subsequently inoculated with virulent blood.

One inoculated with the same amount of the same preserved material two months later died from the primary inoculation, thus showing that even the attenuated virus can be satisfactorily preserved for a considerable period of time.

In the case of another animal which was inoculated intravenously with this virus no result followed. Fifteen days later the same inoculation was repeated. The temperature began to be elevated on the fourteenth day, and it died of horse-sickness seven days later. The primary inoculation in this case, while being ineffectual to induce the disease, had evidently lowered the susceptibility, so that a fresh stimulus, by the same virus, was sufficient to overcome the resistance entirely.

Having recognised that the blood of animals which lived beyond the ordinary period at which horses usually die from horse-sickness was lowered in virulence, I determined to attempt to produce this change *in vitro*.

Having, therefore, prepared bottles containing citrate solution, and having thoroughly sterilised them, selected animals were bled under the most rigid aseptic management, and the blood received in the bottles which, after being replugged, were incubated at a temperature of 102 F during ten days. In one such experiment, out of a total of fourteen bottles, thirteen remained perfectly free from extraneous organisms. Such blood after incubation was then preserved and tested

I found, in this manner, that it was possible to produce an attenuated virus equally suitable for inoculation as that obtained from the donkey or the ox.

While, however, these experiments demonstrated that it was possible to protect horses by repeated inoculations of an attenuated virus, they equally demonstrated the irregularity of action, owing to the varying susceptibility to the disease in its attenuated form which obtains among horses.

Several important facts, however, which were elucidated, are deserving of careful consideration, viz. .—

Death in cases of horse-sickness cannot directly be ascribed to hyperpyrexia, inasmuch as several horses have recovered after having experienced temperatures of over 107 F.; while others, which have died, and, in which characteristic lesions have been found, have not had a temperature exceeding 105 F.

Protection can be arrived at without the production of very great reaction, provided that a number of inoculations are made into the animal, and that these have been so arranged as to proceed very gradually to the highest degree of virulence.

3. It is exceedingly difficult to determine the exact degree of attenuation in any particular sample of an attenuated virus. I have usually attempted this by the inoculation of the virus into one or, at most, two horses; but if the susceptibility of such animals happens to be of a low grade, then the reaction produced may not obtain in other horses for which it may subsequently be used. In other words, to determine

exactly the strength of an attenuated virus it would be necessary always to make the test on at least five animals

4 The indication for future experimentation was thus to call for the discovery of some method by which a virus of standard virulence might be at will reduced to any required degree of attenuation

Experiments were also made to determine whether the blood of an animal suffering from secondary fever had any infective property To this end animals under secondary fever with temperature as high as 106 1 were bled and the blood used to inoculate clean animals, but in no case was any reaction produced thereby I therefore am convinced that the blood during secondary fever is non infective

Experiment with Serum and Defibrinated Blood from Animals which have recovered from Horse-sickness

The experiments made have included serum derived from—

1 Animals formerly salted
2 Animals formerly salted and subsequently reinoculated by periodic injections of gradually increasing doses of virulent blood, the maximum dose being 1000 c c
3 Animals treated as in Clause 2 but subsequently permitted to rest for several months and then reinoculated with a small dose (5 c c) of virulent blood

Under the first clause, serum was obtained from a well ' salted " animal which had been twelve days previous to bleeding, noculated with 5 c c of preserved virulent blood

Animals which were inoculated with 2 c c of virulent blood were subsequently inoculated with large doses of serum (100 c c or more) The inoculations, in some cases began on the day that virulent blood was injected, in others it was delayed until the temperature began to rise, but although the total amounts given exceeded 1000 c c, no definite interference with the course of the disease was noticeable Under Clause 2 ' salted animals were inoculated with progressively increasing doses of virulent blood When these animals had been inoculated with doses of virulent blood equal to 1000 c c they were allowed to rest for eight to twelve days, after which they were bled

Of this serum, 500 c c was inoculated at one dose into a horse, which, during thirty three subsequent days, manifested no signs of illness due to the inoculation When this period was completed, it was inoculated with virulent virus and as a result died of characteristic horse-sickness No evidence was shown that the serum had in any way interfered with the action of the virus. Where, however, this serum was used to inoculate animals which were already infected, a very curious change in the character of the disease occurred.

The animals became affected usually in thirty-six hours with hæmoglobinuria, which later passed into hæmaturia and ended invariably fatally if the disease was virulent. In two cases, however, where the disease had been induced by an attenuated virus, the hæmaturia came to an end with the subsidence of the fever. In all this curious condition was produced by serum in nineteen cases.

Where animals are bled into citrate solution the plasma is of a yellow colour, but in cases which eventually became the subjects of hæmaturia, I noticed if they were bled about twenty-four hours previous to the onset of this condition that the plasma was red coloured. It is therefore evident that the condition has its origin in the blood.

In several cases animals which were partially protected, became subject in a slighter degree to this complication if they were reinoculated with virulent blood and were unable to resist it.

This blackwater may have some relation to the blackwater fever in man. It is generally believed in Rhodesia that blackwater does not occur as a primary disorder but only supervenes in persons who have previously been the victims of malarial fever.

It seemed to me that this serum might in some way be associated with a residual infection. To determine this I inoculated a "salted" horse, which had also had repeated large injections of virulent blood, with 50 c.c. of fresh blood. I bled it eight days later and with 5 c.c. of its blood inoculated a clean animal which thereafter had a very slight rise of temperature on the eighth day.

An animal similarly treated was finally inoculated with 300 c.c. injected intravenously and 20 c.c. subcutaneously thirty-nine days previous to being bled. When bled the blood was defibrinated, and 100 c.c. was injected into each one of six animals. No evidence was shown of any infectivity of the blood.

I now determined to make use of the serum from animals which, under Clause 3, had been allowed to rest for periods over one month previous to the collection of their serum.

This serum is that which is now being used for the purpose of protective inoculation.

I have determined with regard to it—

1. It possesses no curative action which in practice would be of any avail to restrain the course of the disease.

2. Since an injection of 100 c.c., into one animal has absolutely no effect in restraining the action of 1 c.c. of ordinary preserved virus inoculated subcutaneously on the other side, it does not possess any immunising power which would be of practical value in withstanding infection.

3. Its germicidal activity is extremely weak, as is shown by the following experiments:—

(*a*.) 1 c.c. of fresh virulent blood was mixed with 100 c.c. of serum, and after being kept for twenty-four hours in the ice chest was inoculated into a clean horse. The animal had a sharp febrile reaction.

(*b*.) Another animal was treated in the same way, but the serum and blood was injected immediately after being mixed. This animal also had a reaction, but less severe than the former. Variation of susceptibility must of course be taken into account, and, in order to establish this conclusion satisfactorily, a considerable number of animals would require to have been simultaneously dealt with.

(*c*.) Equal volumes of serum and preserved blood were mixed and kept at ordinary room temperature for four days. Of this mixture, 2·5 c.c. was injected subcutaneously into a clean animal. Fever set in after the usual period of incubation, pursued its characteristic course, and the animal died under circumstances and in the usual time which obtains after the use of pure virulent blood.

Since 1 c.c. of virulent blood mixed with 100 c.c. of serum produced a sharp febrile reaction in one animal but had practically no effect in some others, and since 1 c.c. of blood and 200 c.c. of serum produced a reaction in another animal, it was clear that under this method also I should have to meet differences of animal susceptibility.

It was so far fortunate that preserved virulent blood acted equally well as fresh blood, so that a standard virus is easily prepared and maintained, and by mixing the serum of a considerable number of animals I am able to standardise a large volume of serum.

I concluded, therefore, to determine the amount of serum which, when mixed with a definite amount of blood, would serve, acting in concert with the natural protective bodies in the system of the average horse, to ensure the production of the modified disease. After fourteen days should have elapsed subsequent to this inoculation, provided a severe reaction was not set up, I intended to re-inoculate with the same dose of virulent blood, but with a much reduced quantity of serum. Again, after fourteen days, the procedure should be repeated, the dose of virulent blood remaining a constant quantity, but the dose of serum being still further reduced. Finally I intended to inoculate with virulent blood by itself.

In the first three series of experiments sixteen horses were used. These were inoculated as follows:—

1st Inoculation	1 c.c. virus and 100 c.c. serum	(10 animals).	
	1 c.c. „ 90 c.c. „	(4 animals).	
	0·5 c.c. „ 50 c.c. „	(2 animals).	
2nd Inoculation	0·5 c.c. virus in 30 c.c. „		A slight variation of the quantities was made in several cases.
3rd Inoculation	0·5 c.c. „ 15 c.c. „		
4th Inoculation	0·5 c.c. pure preserved virulent blood		

The following shows the results obtained, and where the remark "salted" is made, it is to be understood that the animal has, at later dates, withstood enormous doses of the most virulent blood.

Animal.	Reaction	Result.
1st.	No reaction at all	Salted.
2nd.	Reaction to 1st only	„
3rd.	No reaction	„
4th.	Slight reaction after all four	„
5th.	Reaction to 4th	Died
6th.	Slight reaction to 1st	„
7th.	No reaction	Salted
8th.	Reaction to 4th	Died.
9th.	Slight reaction to 3rd; after 5th...	„
10th.	Slight reaction to 4th	Salted.
11th.	Slight reaction to 4th	„
12th.	Reaction to 5th	Died.
13th.	No reaction	Salted.
14th.	Reaction to 5th................	„
15th.	Reaction to 4th...............	„
16th.	Reaction to 4th...............	„

In the next experiment seven animals were used, which were inoculated as follows:—

Inoculations			
1st.	1 c.c. virus and 100 c.c. serum.		
2nd.	0·5 c.c. „	25 c.c. „	
3rd.	0·5 c.c. „	10 c.c. „	
4th.	0·5 c.c. „	1·5 c.c. „	2 had 0·5 c.c. pure virus.

The results were as follows:—

Animal.	Reaction.		Result.
1st.	Slight reaction to	5th	Salted.
2nd.	„ „	3rd and 5th.	„
3rd.	No „	4th	After a large dose of pure virus, died.

Animal	Reaction.	Result.
4th.	Slight reaction to 2nd	Salted.
5th.	No " 4th .	After a large dose of pure virus, died.
6th.	Slight " 2nd	Salted.
7th.	" " 3rd .	"

Note – Where "5th" is mentioned, it refers to a dose of pure virulent blood.

Total animals inoculated .	23
" " died...	9
" " salted	14

The tests thus applied have been of the most severe character, and despite the fact that these are only of the value of preliminary experiments, the results are extremely satisfactory.

Obviously animals have been sacrificed which, under altered methods, might have been saved, for the outcome of these inoculations goes to show that, unless some reaction has been produced during the earlier reactions, there is no certainty that an animal is protected. Nevertheless it is equally proved that some have become highly protected without having shown any reaction at all.

The indication, therefore, has been to increase the dose of the virus used in the primary inoculations, even at some risk to the more susceptible animals.

In a subsequent series of animals this has been carried out in the following manner:—

1st. Inoculation 2 c.c. virus and 50 c.c. serum.
2nd " 2 c.c. " 20 c.c. "

Twelve animals have been simultaneously inoculated in this manner. The reactions produced have been as follows:—

Animal.	1st Inoculation.	2nd Inoculation.
1	None.	None.
2	Slight.	Slight.
3	"	"
4	"	Severe.
5	Slight.	None.
6	"	"
7	Severe.	Slight.
8	Slight.	"
9	None.	"
10	Slight.	"
11	"	None.
12	"	Severe.

Since one animal, after the first inoculation, had a severe reaction, it is evident that the limit of strength, consistent with safety, had been reached. The reactions, in the two cases, after the second, were extremely severe, and indicate that the limit of strength of virus for that inoculation had been slightly exceeded, if a widespread scheme of operation had been intended to be carried out among animals in the open.

These results would seem to indicate that fortified serum, *e.g.*, that obtained from animals which, after "salting," have been reinoculated with large doses of virus, exerts a peculiar and definite action on the virus.

While, however, 100 c.c. of serum suffices to prevent 1 c.c. of virulent blood, when mixed with it, producing any great elevation of temperature, I have referred to a case in which a severe reaction was produced. Since, in another case, 200 c.c. of the same serum, with an equal amount of virulent blood, was followed by a reaction and a definite amount of protection, it is evident that the difference in susceptibility between the latter animal and those which react slightly after 100 c.c. of serum and 1 c.c. of virulent blood is equal to 100 c.c. of fortified serum. Moreover, as already shown, when the virus is attenuated by its passage through less susceptible animals, such as the donkey or cow, its effect, when used in the same dose, either by subcutaneous or intravenous injection, varies very greatly in different animals; in some producing no evident reaction, in others setting up some fever; while, again in others, its use was followed by the onset of the virulent disease resulting in death.

If, therefore, the admixture of serum with virulent blood is followed, on inoculation, merely by a modified form of the disease, it must be concluded that the serum, of itself, cannot be credited with this result, but that a peculiar quality, existing in the animal body, and varying in amount from animal to animal, must play an important part. Whether this principle is a simple body, or is a combination of several, cannot at this moment be determined, but for convenience' sake I would suggest that the name "Antagones" should be applied to it. The term need not be taken to imply either an antitoxic or germicidal body, but merely to denote the "defensive" properties which are already existent to a greater or less degree in all animals, or are produced or increased under special stimulation.

Since thoroughly "salted" animals and donkeys can be reinoculated and infection proved to exist in their blood for at least ten days subsequent, I am inclined to look upon the protection existing in "salted" animals as of the nature of a "tolerance," and to believe that true immunity, in horses, against this disease is never acquired.

"Note on the Occurrence of a Seed-like Fructification in certain Palæozoic Lycopods"

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It has generally been assumed by palæobotanists that the fossil seeds described by Williamson[*] under the name of *Cardiocarpon*, even if not necessarily co-generic with the *Cardiocarpus*[†] of Brongniart, at least belonged to the same group of Gymnospermous plants.[‡] Brongniart's specimens, often preserved with marvellous perfection, have proved to be the seeds of members of the extinct Order Cordaiteæ, or of allied plants. The same conclusion applies to certain of the British forms, notably the *Cardiocarpon anomalum* of Carruthers,[§] which was certainly Cordaitean, and probably to some of Williamson's examples.

The specimens to be shortly described in the present note show, however, that seed-like bodies, identical with those figured by Williamson under the name of *Cardiocarpon anomalum*,[||] were borne on Lepidodendroid cones, otherwise indistinguishable from *Lepidostrobus*. They thus prove that under the genus *Cardiocarpon*, and even under the "species" *C. anomalum*, totally different objects have been confounded, namely, the seeds of Cordaiteæ or Cycads on the one hand, and the integumented megasporangia of certain Palæozoic Lycopods on the other. The latter organs present close analogies with true seeds, but are wholly distinct in detailed structure from the Gymnospermous seeds above mentioned.

The discovery of the specimens of the new cone is due to Messrs. J. Lomax and G. Wild, who recognised it as a *Cardiocarpon*-bearing strobilus, resembling a *Lepidostrobus*.[¶]

The original specimens, which are calcified and generally well preserved, were derived from the Ganister beds of the Lower Coal-measures, some from Hough Hill, Stalybridge, others from Moorside, Oldham. Numerous sections were cut by Mr. Lomax and Mr. Wild. A closely similar fructification occurs, at a much lower horizon, in the Burntisland beds of the Calciferous Sandstone Series.

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[*] "Organisation of the Fossil Plants of the Coal-measures," Part VIII, 'Phil. Trans.,' vol. 167, Part I, 1877, p. 254

[†] Founded in Brongniart's 'Prodrome d'une Histoire des Végétaux Fossiles,' 1828. The forms *Cardiocarpon* and *Cardiocarpus* have been used indiscriminately by authors.

[‡] See, for example, Solms-Laubach, 'Introduction to Fossil Botany' English edition, p. 120.

[§] "Notes on some Fossil Plants," 'Geol. Mag.,' vol. 9. 1872.

[||] *Loc. cit.*, Part VIII, Plate 14, fig. 118, and Plate 16, fig. 119; Part X, 1880, Plate 20, fig. 64. These figures are from specimens which I have certainly identified with the Lepidostroboid fructification. Others figured by Williamson are of doubtful nature.

[¶] See the note by Messrs. Wild and Lomax, "On a new *Cardiocarpon*-bearing Strobilus," 'Annals of Botany,' March, 1900.

The strobilus is of the ordinary *Lepidostrobus* type. The cylindrical axis bears numerous spirally disposed sporophylls, each of which consists of a long horizontal pedicel, expanding at the distal end into a rather thick lamina, which turns vertically upwards.

Anatomically, the structure is also that of a *Lepidostrobus*. The stele which traverses the axis has a narrow ring of centripetal wood, and a large pith; the leaf-trace bundles which pass out to the sporophylls are collateral in structure, and agree closely with those described by Mr. Maslen in *Lepidostrobus Oldhamius*.*

The ligule is sometimes well preserved, it is seated in a depression of the upper surface of the sporophyll, at the distal end of the sporangium, and is thus in the normal position.†

With one exception, the specimens of the strobilus are immature, and their tissues not quite fully differentiated. These younger specimens bear sporangia which are essentially those of a *Lepidostrobus*. A single large sporangium is seated on the upper surface of the horizontal pedicel of each sporophyll, to the median line of which it is attached along almost its whole length.

The sporangium narrows out towards the top, and terminates above in a well-marked ridge, in general form it resembles Williamson's *Cardiocarpon anomalum*, but in the immature condition there is no integument. The outer layer of the sporangial wall has the columnar or palisade like structure characteristic of *Lepidostrobus*, it is lined by a more delicate inner layer, which may be several cells thick.

So far the structure is simply that of a *Lepidostrobus* with rather thick-walled sporangia.

Within the sporangial cavity, the membranes of the megaspores are usually preserved; a single large megaspore almost fills the sporangium, but smaller, abortive spores, with thicker walls, are also present. Some specimens show that three of these abortive spores were present in each sporangium. It appears, then, that a single tetrad was developed in each megasporangium, and that of the four sister-cells one only came to perfection, constituting the functional megaspore.

In one specimen, discovered by Mr. Wild, the strobilus is in a more advanced condition. In its upper part the sporophylls simply bear sporangia, as above described, but lower down in the cone these are replaced by integumented, seed-like structures, identical with the detached bodies called *Cardiocarpon anomalum* by Williamson.

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* Maslen, "The Structure of *Lepidostrobus*," 'Trans. Linn. Soc.,' London, Ser. 2, vol. 5, 1899.

† Maslen, "The Ligule in *Lepidostrobus*," 'Annals of Botany,' vol. 12, 1898.

The structure of this strobilus is sufficiently well preserved to show that the anatomy of the axis agrees with that of the less mature specimens, and, as the tissues are more completely formed, exhibits the Lepidostroboid characters even more clearly

Mr. Wild's specimen, then, demonstrates that the *Cardiocarpon anomalum* of Williamson was borne on a cone with all the characters of a *Lepidostrobus*, and that it represents the matured condition of the megasporangium and sporophyll.

The detailed comparison of specimens in the young and the mature condition has shown the nature of the change, which converts the megasporangium, together with its sporophyll, into a seed like organ The nucellus of the latter retains almost unaltered the structure of the megasporangial wall, with its columnar layer. In the sporangial cavity the single large megaspore, accompanied by its abortive sister cells, is present as before A thick integument has, however, grown up from the sporophyll, completely overarching the megasporangium, except for a narrow crevice left open at the top When seen in a section tangential to the strobilus as a whole, this crevice is cut across, and presents exactly the appearance of a micropyle; in reality it differs from a micropyle in being a narrow slit, extending almost the whole length of the sporangium, in the radial direction, whereas the micropyle of an ordinary seed is a more or less tubular passage.

The integument springs from the upper surface of the sporophyll-pedicel; it does not consist of the incurved margin of the pedicel, for, in the more distal region, the margin of the latter projects considerably beyond the insertion of the integument.

From the frequency of detached specimens in the *Cardiocarpon* condition, it appears that in nature the sporophyll, bearing the integumented megasporangium, was shed as a whole, though parts of the sporophyll-lamina no doubt perished, only so much being persistent as was necessary to form a complete envelope to the "seed."

In a strobilus associated with the seed-like specimens, and bearing microsporangia, it was found that the latter, like the megasporangia of the female cone, are provided with integuments. This specimen was figured by Mr. Maslen as a variety of *Lepidostrobus Oldhamius*,* though possibly deserving specific rank, a determination with which I agreed at the time. There is every reason, however, to suppose that this strobilus was a male fructification of the same species, the female of which bears the integumented seed-like megasporangia above described. The microsporangial integument is more widely open than that of the megasporangium.

The Burntisland specimens, which from their horizon are presumably of a distinct species, are at present only known in the isolated *Cardiocarpon* condition. They are of interest for two reasons: in one specimen the ligule is clearly shown, enclosed by the integument, the only example of this organ, so far observed, in the mature, seed like stage of the fructification

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* Maslen, 'Structure of *Lepidostrobus*,' p. 371, Plate 37, fig. 21.

Another of the Burntisland specimens is the only one as yet observed in which the prothallus is present * It fills a great part of the functional megaspore, which is almost co extensive with the sporangial cavity, and consists of a large celled tissue, resembling the prothallus of *Isoetes* or *Selaginella* The peripheral prothallial cells are smaller than the rest, but no archegonia could be detected

The bodies described in this note resemble true seeds in the possession of a testa or integument and in the fact that one megaspore or embryo-sac alone came to perfection the seed like organ was likewise shed entire, and appears to have been indehiscent In many points of detail, however, the reproductive bodies in question differ from the seeds of any known Gymnosperms, they afford no proof of the origin of the latter Class from the Lycopods The newly discovered fructification nevertheless shows that certain Palaeozoic Lycopods with strobili at first indistinguishable from *Lepidostrobus*, crossed the boundary line which we are accustomed to draw between Sporophyta and Spermophyta

As these fossils appear worthy of generic rank I propose to found the genus *Lepidocarpon* for their reception, it may be briefly characterized as follows —

Lepidocarpon, *gen nov* —Strobilus, with the characters of *Lepidostrobus*, but microsporangia and megasporangia each surrounded by an integument, growing up from the upper surface of the sporophyll Megasporangium completely enclosed in the integument, except for a slit-like micropyle along the top A single functional megaspore developed in each megasporangium Sporophyll, together with the integumented megasporangium, detached entire from the strobilus, the whole forming a closed, seed like, reproductive body

It is proposed to name the Coal measure form *Lepidocarpon Lomaxi*, and that from Burntisland *L Wildianum* Both were included by Williamson under his *Cardiocarpon anomalum*, which, however, is quite different from the seed so named by Carruthers

A full, illustrated account of these fossils is in preparation, and will shortly be submitted to the Royal Society

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* I have since examined a section, cut by Mr Lomax from one of the Coal-measure specimens, in which the prothallus is even better preserved —*Note, added October 9, 1900*

"The Demarcation Current of Mammalian Nerve (Preliminary Communication) I The Demarcation Current of Mammalian Nerve"

By J S MACDONALD B A L R C P E, University College, Liverpool, Research Scholar of the British Medical Association Communicated by Professor SHERRINGTON, F R S Received July 28 1900

1 A necessary preliminary to the study of the distribution of the demarcation current and source is an examination of the character of the resistance of the particular nerves in which phenomena are observed

The resistance per cm of the nerves examined—vagus, phrenic, and sciatic nerves of dog and cat, &c, varies with the nerve and with the animal

Vagus	Horse, 2000 ohms per cm
"	Dog, 12,500 "
"	Cat, 31,000 , "
Sciatic	Dog, 3500
"	Cat, 4500

the variations depending upon the character of the nerve and presumably of the individual fibres, upon its sectional area, and probably upon intrinsic differences between the average salt content of the tissues of different animals

But taking any individual nerve the estimated value of the resistance per cm varies with the length of the piece, from the determination of the resistance of which the estimation is made

Thus keeping one electrode at a fixed point of a nerve, the cross-section A, moving a second electrode from point to point, B, C, D, &c, and measuring the resistance included between A and these several other points, a series of determinations of the resistance of various lengths of the same nerve are made From a knowledge of the length of the piece and from this determination, an estimation of the resistance per cm of the nerve is obtained which is greater with each diminution of length

Experiment —Vagus Nerve of Dog

Piece	Length	Resistance	Resistance per cm
AB	15 mm	29,500 ohms	19,700 ohms
AC	30 "	58,500 ,	19, 00 "
AD	50 "	85,000 ,	17,000 "
AE	65 "	108,000 "	16,600 "
AF	83 "	133,500 "	16,080 "

The calculated resistance per cm is also considerably greater when the resistance is measured from one point on the longitudinal surface to another than in the case in which the resistance of a similar length is taken from the cross section to a point on the longitudinal surface and greater still than the resistance of a similar length bounded by two cross sections

When therefore a knowledge of the resistance is required as a basis for calculations a direct determination of the resistance is only of value when the resistance is required to a current having the same path as that used for the measurement of resistance

In any other case as when the fraction of the longitudinal resistance corresponding to a fraction of the length of the nerve is required it is better to use a calculated value than the value of the directly determined resistance This calculated value is best obtained from the resistance per cm of the longest available stretch of the nerve

* * * * *

2 If a point on the longitudinal surface is connected to the cross section through a pair of non polarisable electrodes and an outer wire path and a current is found to traverse the circuit so formed then the current can not only be found in the outer wire path by means of a galvanometer placed in it but can also be followed in a return direction in the nerve travelling in the opposite direction from the cross section to the longitudinal surface by means of the new differences of potential which the formation of the outer circuit immediately establishes in every intervening point of the nerve

This is true when any arbitrarily selected point on the longitudinal surface is connected to the cross section In each case the return current through the nerve is found established as a new phenomenon, due to the closure of the outer path, and is exactly the current due to the action of a source of E M F of the value determined as the potential difference between the point on the longitudinal surface and the cross section in a circuit of the resistance found

Experiment —Vagus Nerve of Cat

The nerve was excised and laid upon four non polarisable electrodes, A, B, C, D The cross section was at A the nerve stretched a small distance beyond electrode D, and was suspended to the wall of the moist chamber by a silk thread

The potential difference (so called) between points A and D was determined as 0 00712 volt

The potential difference between the two intermediate points B and C was zero, the points being equipotential

Points A and D were now permanently connected through the resistance of a pair of non-polarisable electrodes and a wire joining them, forming in all a circuit of resistance 135,000 ohms.

Nerve AD . ... = 128,000 ohms.
Electrodes . ... = 7000 ohms.
The length of nerve AD = 4·3 cm.
„ „ piece BC = 1·8 cm.

The "calculated value" of resistance B, C = 54,000 ohms.

The value directly determined = 63,000 ohms.

After closure of circuit A, D, the intermediate point B, nearest to the cross section, became "+" to the more distant intermediate point C, which was "−".

The value of this difference was 0·0028 volt.

If in this experiment it is assumed that, (1) the only source of E.M.F. is that found and measured as the potential difference between A and D, 0·00712 volt and (2) that the path of the current is the simple one of nerve electrodes and wire, *i.e.*, the path through the nerve is simple, and not divided into two sets of resistances carrying a current in opposite directions (circuit completed in nerve itself): then a difference of potential should be found between points B and C of this path

$$= \frac{r}{R} E,$$

$$= \frac{54,000}{135,000} \, 0·00712 \text{ volt},$$

$$= 0·00284 \text{ volt},$$

and this is practically the value actually found.

In many similar experiments which have been performed, this agreement of value found and value calculated has been found to hold good within a small limit of error, entirely owing to an alteration of the E.M.F. due to the cross section and to the lapse of time taken to perform the experiment.

When there is a pre-existing difference of potential between the points B and C, this difference subtracts from the newly-acquired value due to the closure of the circuit A, D, and the value actually found is the algebraical sum of the pre-existing and the newly-acquired difference.

Since a pre-existing difference between B and C is the source of the "longitudinal current," the last point in the above statement is considered of importance, as tending to show that the source of the demarcation current and that of the longitudinal current can be so separable as to oppose one another in a conveniently arranged circuit.

3 Similar experiments show that the closure of a circuit for the observation of a *longitudinal current* also gives rise to similar changes of potential in the intervening stretch of nerve

Similar experiments show that the closure of a circuit for the observation of *an electrotonic current* affects the potential of every intervening point in the same way

So that if a path joining two points on a nerve is found to be carrying a current whether it be demarcation longitudinal or electrotonic this current can be traced in the longitudinal axis of the nerve making use of its gross longitudinal resistance and not interfered with by currents from any other of the possible sources of E M F, discovered by the determination of pre existing differences of potential of intervening points None of these sources are brought into action so as to affect a current in the longitudinal axis of the nerve until an additional outer circuit of non polarisable electrodes and wire is formed for them they then at once are brought into action and add to or subtract from the original

* * * * *

4 Long stretches of mammalian nerve—vagus sciatic and phrenic—of about 10 cm long have been taken and laid upon an ebonite scale The E M F s available between either cross section and the points distant 1 2 3 4 &c cm *from one end* have been systematically measured and from the measurements curves obtained with the nerve as abscissa and the ordinates the E M F between the immediately underlying point of longitudinal surface of the nerve and the cross section

Whatever be the difference between the E M F s due to the two cross sections (in some cases a difference of height of maxima of 0 006 volt the lower one *e g* being 0 002 volt the higher 0 008 volt), and whatever be the peculiarities of the curves they are notwithstanding the difference of level parallel for the greater portion of their extent The curve due to the available force between one cross section and the longitudinal surface repeats all the maxima and minima of the curve due to the other cross section at a different level, the relations between the maxima and minima being preserved unaltered

Such a condition of affairs is most readily explained by the assumption that the determination of E M F between a point on the longitudinal surface and the cross section is always the determination of the algebraical sum of two opposing forces, one acting radially at the point on the longitudinal surface, and one at the cross section acting in the longitudinal axis of the nerve and that the radial force remains the same at a point of the longitudinal surface, whereas there may be and usually is a difference between the two longitudinal forces, one at each cross section

* * * * *

5. Selecting one cross section and systematically measuring the E.M.F. available between this and points on the longitudinal surface, drawing a complete curve (force diagram) at regular intervals of 30′ between each curve, the levels of the curves fall with diminishing rapidity, and a gradual change occurs in the form of each curve.

After the nerve has been removed some hours from the animal, the maximum E.M.F., the highest point of the curve, may have sunk to one-tenth of its original value. If now the nerve is taken and placed for a short period (five minutes) in tap water, a maximum E M F. is obtained considerably greater than the maximum obtained from any point of the same nerve when freshly removed from the recently-killed animal. If the value of the demarcation source is taken as an index of the condition of the nerve, then a rejuvenation has taken place with the immersion in tap water.

This increased value remains for some time, the curve being of not much different form, and the rate of fall of level being similar to the rate of fall from the original maximum. If the nerve be left in tap water for twenty-four hours, a demarcation current, a difference of potential between each point on the longitudinal surface and the cross section is observed, giving a curve very similar to that obtained after the first immersion in tap water twenty-four hours previously.

If one waits after the death of an animal until *rigor mortis* is completely established, and a nerve be then removed, only a small trace of demarcation current is obtainable from it, and the curve of E.M.F. due to a cross section is at an extremely low level. If now this nerve is left for a short period (five minutes) in tap water, a maximum E.M.F. is obtainable from it higher than that obtained from the fresh nerve of the recently-killed animal, and as high as that obtained after the immersion of the fresh nerve in tap water, and there is no marked difference in the form of curve (force diagram).

If a nerve is removed at once after the death of an animal, and the E.M.F. between a "maximal" point and the cross section is taken (1) immediately (2) after a short immersion in 0·9 per cent. saline, then it is seen that the 0·9 per cent. (normal) saline has diminished the E.M.F. If the nerve be now immersed in 0·45 per cent. saline the original value is returned, and is increased by a further immersion in 0·3 per cent., 0·2 per cent., 0·1 per cent. saline solution, each further dilution increasing the E.M.F. A return of the nerve through the series diminishes in each case the E.M.F., to be renewed by a subsequent return to the weaker solution.

The maximum effect is obtained by the action of tap water, and a very considerable reduction of this maximum is obtained by a subsequent immersion in 0·1 per cent. saline, to be followed by a return to the maximum with a return to the tap water.

There is no sign of any critical point marking the separation of two possible *phenomena, one a function of the condition of life of the nerve and the other a physical phenomenon dominated by the salt content of the nerve and capable of continuation long after its death*

* * * * *

6 If a number of threads are twisted together to form a rope and the rope laid upon two non polarisable electrodes of the usual type no current is found between the electrodes if the thread rope is previously uniformly wetted with a saline solution or with tap water If on such an uniformly wetted rope a drop of saline solution of a different concentration is placed at a point closer to one electrode than another a current is found in the circuit and a source of E M F quite comparable in value to the maximal value of the demarcation source of a nerve A drop of the same solution placed upon a corresponding point of the rope nearer to the other electrode may reduce bring to zero, or reverse this difference of potential

This phenomenon is presumably due to the upsetting of the balance between the osmotic processes taking place in the two non polarisable and 'similar' electrodes

* * * * *

7 The close association of the value of the demarcation current with the salt content of the nerve suggests a similarity between the experimental phenomena observed in the thread and in the nerve, and the causation of the demarcation current of nerve as due to a balance between two unequal osmotic processes one at the cross section and one at the longitudinal surface

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The expenses of this research have been partially defrayed by a grant from the British Association

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The Demarcation Current of Mammalian Nerve (Preliminary Communication) II The Source of the Demarcation Current considered as a Concentration Cell

By J S MACDONALD B A L R C P E University College Liverpool, Research Scholar of the British Medical Association Communicated by Professor SHERRINGTON, F R S Received September 25, 1900

The changes produced by the action of tap water upon the nerve have, in the interval, been more closely studied Excluding alterations of E.M F, they are as follows —

An increase of
- (a) weight.
- (b) length.
- (c) rigidity.
- (d) elasticity.

A diminution of
- (a) compressibility.
- (b) conductivity.

Thus a piece of sciatic nerve (cat) examined before and after an mersion of 20 minutes in tap water:—

	Before.	After.
Weight in grammes	0·237	0·317
Length in centimetres	4·8	5·4
Resistance in ohms	14,200	18,400

The general condition is known, and has been previously described as "water rigor"; a similar change produced by the action of water upon muscle has been found (see Fletcher*) to be unattended by the chemical changes accompanying other forms of rigor.

With the exception of the change in conductivity, all the changes are rapidly developed, reaching their maxima within an hour after immersion in water; they may remain for days in an approximately maximal condition, but are at any time abolished by a short immersion in 0·9 per cent "saline." The change in conductivity is a much more gradual one, as is well shown by the details of an experiment. In the following experiment the appearances of rigor were fully developed within the first hour, the available E.M.F. had also risen to its maximum (0·027 volt as contrasted with the initial value 0·018 volt), whereas the nerve still retained 70 per cent. of its original conductivity.

Experiment.—Sciatic Nerve of Dog.

Length of piece used, 5 cm.

The initial resistance having been measured, the nerve was placed in tap water (1 litre) from which it was removed every twenty-five minutes for re-examination.

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* 'Journal of Physiology,' vol. 23, i, ii, 85.

	Resistance.	
	In ohms.	In terms of the original as unity.
Nerve at first	15,900	1·00
½ hour	20,400	1·28
1 "	23,100	1·45
1½ "	28,800	1·81
2 "	39,800	2·50
2½ "	51,100	3·21
3 "	54,900	3·45
3½ "	61,000	3·84
4 "	88,700	5·58
* *	*	*
24 hours	307,500	19·34

The condition of rigor is most completely produced by the action of water, but in all the characteristics observed differs only in degree from similar conditions produced by dilute saline solutions, *e.g.*, 0·6 gramme per cent. NaCl solution.

In the latter we get the same increased rigidity, weight, length, &c., as also the diminution of conductivity; all of these, however, are found to a smaller measure, and are much less persistent.

Although not provided with the evidence of direct analysis, it seems very reasonable to assume that both the extreme and the lesser variations, as judged by the character of the changes observed, are to be explained by the occurrence of "osmosis" and "diffusion"; and that in the extreme case, the nerve continues to acquire water until a very considerable internal pressure is produced, thereby causing the extreme changes of form and elasticity, whereas the conductivity is gradually diminished through an increasing loss of electrolytes by processes of diffusion.

It is worthy of note that the appearance is quite different in character and degree from the slight rigidity change which ordinarily, in nerves, follows the death of an animal, and that it can be produced by a short immersion in tap water in a nerve removed from the body of a "several days dead" dog, or in a nerve which has lain for days in saline solutions, and therefore is in no way dependent upon the living state of any of the structures of the nerve. Finally, as a minor proof that it is not produced by the precipitation of globulins in the nerve, it is completely and at once abolished by immersion in a saturated solution of magnesium sulphate.

* * * * *

The graduated character of the changes produced and their parallelism with the graduated concentration of the solutions has been carefully followed out; for the sake of brevity, however, data are given here only from the three experiments which seem to determine the strength of the isotonic solution as showing a minor degree, an absence, and a reversal of the condition.

Experiment A.—Sciatic Nerve of Cat.

The nerve was examined (A) at once, and 1, 2, 3, &c, after successive immersions of ten minutes' duration in a 0 6 gramme per cent. NaCl solution

	A.	(1)	(2)	(3)	(4)	(5)
Ohms . .	19,200	22,600	21,200	20,900	20,600	21,700
Volts .. .	0·018	0 023	0·020	0·016	0·018	0 011
Grammes ..	0 207	0·213	0·221	0·227	0 233	0 236
Centimetres	4 9	5 1	5 1	5 1	5 1	5 1

An error is evidently introduced into the measurement of resistance, if we wish to consider the resistance of a cylinder of the nerve of constant length and cross section, by the increase in volume attending the increase of weight.

Failing an actual determination of the area of the cross section, or one of the volume from which it might be directly obtained by dividing by the length, the value of the weight found in grammes has been treated as volume in cubic centimetres, and used in this way. The error introduced by a neglect of the specific gravity is not, in the case considered, appreciable.

The "specific resistances" for unit length and cross section found in this way from the figures given are—

A.	(1.)	(2)	(3)	(4)	(5)
165	184	180	182	184	188 ohms

Experiment B.—Sciatic Nerve of Cat.

The experiment was in every way similar to the last, with the single exception that the solution used was an NaCl solution of 0·75 gramme per cent.

	A.	(1)	(2.)	(3)	(4)	(5)
Ohms	20,700	20,600	20,200	19,4 0	18,900	19,200
Volts.......... .	0 018	0·018	0·018	0·017	0·015	0·014
Grammes	0·210	0·218	0·222	0·228	0·229	0·229
Centimetres	4 6	4·7	4·7	4 7	4·7	4 7
"Specific resistances"	205	203	203	200	196	196

Experiment C—Sciatic Nerve of Cat

Similar experiment Solution used 0 9 gramme per cent NaCl

	A	(1)	(2)	(3)	(4)	(5)
Ohms	20 300	18 700	16 700	16 900	17 100	16 1[illegible]0
Volts	0 014	0 014	0 011	0 009	0 009	0 009
Grammes	0 286	0 247	0 2[illegible]7	0 262	0 260	0 280
Centimetres	5	5	5	5	5	5
Specific resistances	191	184	171	177	177	167

Taking the three experiments and examining the variations in the specific resistance in each case it is noticeably least affected in the 0 75 per cent solution which therefore from this point of view most nearly approaches the isotonic solution

Reducing the data from the three experiments to an assumed average value of 200 ohms for the initial resistance we have

	Initial resistance	After successive immersions in the solutions				
		(1)	(2)	(3)	(4)	(5)
Experiment with 0 6 per cent	200	222	218	220	222	228
0 75	200	198	198	194	190	194
0 9	200	192	178	184	184	174

or, taking averages from the five determinations made after immersion in the solution,

	Initial	Final specific resistance	Total increase
0 6 per cent	200	225	+25 ohms
0 75	200	195	−5
0 9	200	185	−15

The alteration in weight in the three experiments is apparently anomalous, as there is an increase in each case, and judged from this criterion alone, none of the solutions are isotonic Considerable care was taken to dry off the superficial moisture, and as far as possible the precautions were the same in each case There is a difference in the character of the increase which is confirmed by the results of other experiments, the increase in the hypotonic solution is progressive, in the other two there is an increase to, and a maintenance of, a steady maximal weight. It seems evident that in all three cases there is an imbibition of saline solution at first, apart from any question of the transference of water through a membrane.

The appearances of rigidity were noticeable in the nerve immersed in the 0·6 per cent. solution, though the only index of their occurrence found in the figures given is provided by the increase in length. The occurrence of the appearances is, however, unmistakable in an actual experiment, the difference between a nerve which has been immersed in 0·9 per cent. and one which has been in 0·6 per cent. is quite a marked one.

* * * * *

Markedly graduated as are the changes of weight, length, resistance, and rigidity following the immersion of nerves in solutions of graduated concentration, the changes in the E.M.F. available between cross-section and longitudinal surface are no less so. In so far is this true that it is possible, knowing the initial value of the E.M.F., to predict the value which will be found after immersion of the nerve for a given time in a solution of known concentration at a constant temperature.

The accompanying data are taken from the records of 8 entirely separate experiments. In each, a sciatic nerve (cat), removed immediately after the death of the animal, was cut to a definite length of 5 cm., and placed for 25 minutes in 500 c.c. of an NaCl solution, at a temperature of 17° C.

The E.M.F. found after immersion is given expressed in terms of the initial E.M.F. used as unity, is called the E.M.F. "recovered," and as will be seen is sometimes greater than the initial value.

Solution used.	Proportion of E.M.F. recovered.
(1) Tap water	1·5
(2) 0·6 gramme NaCl per cent.	1·059
(3) 0·75 „ „ „	0·921
(4) 0·9 „ „ „	0·786
(5) 1·8 grammes „ „	0·388
(6) 3·0 „ „ „	0·237
(7) 6·0 „ „ „	0·107
(8) 9·0 „ „ „	0·062

If in each case the value of the concentration of the NaCl solution is multiplied by the E.M.F. recovered (expressed in terms of the initial E.M.F.), we get a value which is almost constant for the whole series.

(2) 0 6 × 1 059 = 0 6354
(3) 0 75 × 0 921 = 0 69075
(4) 0 9 × 0 786 = 0 7074
(5) 1 8 × 0 388 – 0 6984
(6) 3 0 × 0 237 – 0 7110
(7) 6 0 × 0 107 0 6420
(8) 9 0 × 0 062 0 5580

Which relationship can be interpreted to mean that for a considerable range of concentrations the E M F recovered varies almost exactly inversely as the concentration and outside this range the deviation from the law is not great

The value of the constant which is practically 0 70 is evidently the concentration of a saline solution in which the E M F should be unaltered by the immersion

The preservation of a constant temperature throughout the series of experiments is of importance the variation with temperature being considerable and complicated Data are given from a few experiments made to determine the interest of this point

The data are as before each from a separate experiment upon a measured length (5 cms) of the sciatic nerve of a cat the only difference being in fact that the temperatures of the solutions were varied instead of their concentrations

Temperature of solution	Concentration of solution	E M F recovered in terms of the original as unity
9 C	75 per cent NaCl	1 01
17		0 92
28		0 62
38		0 62
*	*	*
9 C	3 0 per cent NaCl	0 25
17		0 24
27		0 11
34		0 04
2		0 15

It may seem an obvious and foregone conclusion that the isotonic solution in which the nerve may lie with the minimal disturbance due to the transference of water and salts should closely coincide with the solution in which the E M F is retained constant, and also with the probable isotonicity of the solution which in the living body bathed the outer surface of the nerve

It may indeed be maintained that from the point of view of the established hypothesis the local short circuiting of the demarcation source would be affected by solutions varying in concentration from the "normal saline" in just such a way as to cause exactly the variations described in the apparent value of the E.M.F., and that the variations would be connected by the simple law found.

If so, an examination of the alterations taking place in the same "normal solution" when its temperature is varied presents an anomaly for explanation The solution maintained at anything approaching the temperature of the body, in which the E.M.F. would remain constant, is not 0·70 per cent., but 0·45 per cent. NaCl solution.

A more striking anomaly still is obtained when an appeal is made to solutions of electrolytes other than NaCl; an extreme instance is given by the consideration of solutions of NaOH.

The following data are taken from four separate experiments in which 5 cm. pieces of sciatic nerves (cat), removed immediately after death, were placed in each case in 500 c.c of an NaOH solution at a temperature of 17° C., and left in it for 25 minutes.

Solution.	E.M.F "recovered."
(1) 0·025 gramme per cent. NaOH.	1·620
(2) 0·050 ,, ,, ,,	0·922
(3) 0·050 ,, ,, ,,	0·800
(4) 0·100 ,, ,, ,,	0·422

Proceeding as before, and multiplying the concentration by the E.M.F., we have

(1) $0{\cdot}025 \times 1{\cdot}620 = 0{\cdot}0405$
(2) $0{\cdot}050 \times 0{\cdot}922 = 0{\cdot}0461$
(3) $0{\cdot}050 \times 0{\cdot}800 = 0{\cdot}0400$
(4) $0{\cdot}100 \times 0{\cdot}422 = 0{\cdot}0422$.

The concentration law is the same as for NaCl, but the "constant" solution is 0·04 instead of 0·7. Dividing these figures by the molecular weights of NaOH and NaCl respectively, the proportion existing between them is 1 to 12; and this, even allowing for the greater conductivity of NaOH solutions, is evidently a relation of a more complex kind than that found when passing from one concentration to another of a solution of the same electrolyte.

* * * * *

In certain types of experiments, such as those in which the effect of tap water was studied (*vide supra*) upon the nerves of animals in a state of *rigor mortis*, the capacity of the established hypothesis to explain the facts is strained to an absurd degree. The following are brief descriptions of typical experiments concerning which the same statement may safely be made:—

I *Experiment Sciatic Nerve of Cat*

Piece 5 cm long Difference of potential taken between the cross section and a point 1 cm distant

	Volt
At once	0 015
After 25 in 9 per cent NaCl solution at 17 C	0 001
Another 25	0 001
Another 25	0 001
Another 10	0 001
After 15 in tap water at 17 C	0 011
After another 30 in tap water at 17 C	0 022

One hour and 40 minutes in the very abnormal saline solution of 9 per cent NaCl and a subsequent 45 minutes in the very abnormal saline solution of tap water and yet the vigour of the changes is unimpaired—they are displayed to the same abnormal degree by the effects of tap water upon the local circuit *and the contrasted states of activity and hyperactivity are shown in exactly the same situations*

II *Experiments upon Degenerated Nerve*

In these experiments the preliminary operations for section of the nerves were performed by Professor Sherrington F R S They are described in the briefest possible manner but as the nerves were made the subject of systematic and detailed study the full description is withheld as in case of the other experiments for an opportunity for more detailed publication

Experiment a Vagus of Dog

Preliminary Operation —1 cm of nerve excised at upper limit and 1 cm of nerve excised at the lower limit of the nerve in the neck

Examination nine days afterwards

Degenerated Nerve	Intact vagus of other side
E M F = 0 000 volt	E M F = 0 006 volt
Several cross-sections were tried	
After an immersion of 25 in tap water—	After an immersion of 25 in tap water—
E M F = 0 017 volt	E M F = 0 016 volt

Experiment β. Sciatic Nerve of Dog.

Preliminary Operation.—1 cm. of nerve excised.

Examination twelve days afterwards.

Degenerated Sciatic.	Intact Sciatic.
E.M.F. = 0·003 volt.	E.M.F. = 0·017 volt.
After immersion in tap water.	After immersion in tap water.
E.M.F. = 0·025 volt.	E.M.F. = 0·026 volt.

* * * * *

It seems highly probable to the author, biased by the simplicity of the "concentration law," that the extreme case studied, namely, the nerve after immersion in tap water, is but an extreme variation of a pre-existing condition—in fact, that the internal structures of the nerve form what is to all intents and purposes a stronger aqueous solution of electrolytes than is found in its superficial parts, just such an arrangement of solutions as the character of the resistance and internal polarisation of nerves has always made probable.

If this is true, all the arguments which can be adduced to explain the E.M.F. obtained from the extreme case can be transferred, when modified, to the normal condition.

In this extreme case there is no need to invoke a difference in the distribution of the dissociation phenomena of life to explain the existence of a source of E.M.F. The source is granted as soon as it is determined that solutions of different concentration, such as are present, are asymmetrically placed in the otherwise symmetrical arrangement of solutions connecting the metallic electrodes.* Failing an absolute knowledge of this asymmetry, there are many reasons which make it highly probable; the anatomical conditions are obviously asymmetrical.

The mathematical considerations determining the value of such a source have been so perfectly elaborated, and consequently simplified, that the data collected from the examination of a supposed instance, even of a complicated case, can be afforded a criticism of great exactness.

With a view to such criticism the research is being continued, and for the present the conditions of a possible reversal are sought.

Throughout the conduct of this research I have been most liberally assisted with information and advice by Professor C. S. Sherrington, F.R.S., and Professor Oliver Lodge, F.R.S., for which I take this opportunity for expressing my gratitude. I have also to thank Mr. B. Davies, Mr. A. Hay, and Mr. W. H. Derriman for their frequently sought opinions, and Mr. W. G. Lloyd for practical assistance in some of the experiments.

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* Concentration cells of Nernst, 'Electrochemistry,' Le Blanc.

"The Demarcation Current of Mammalian Nerve (Preliminary Communication) III The Demarcation Source and the Concentration Law" By J S MACDONALD B A L R C P E, University College Liverpool Research Scholar of the British Medical Association Communicated by Professor SHERRINGTON F R S Received October 18 1900

Since writing the previous statement the changes of E M F occurring during the early part of the time spent in saline solutions have been studied more in detail, the nerves being removed every five minutes for examination

As a result of information so acquired the following statement can be definitely made Solutions of NaOH, HCl NaCl, KCl mainly affect the demonstrable value of the demarcation source according to their concentration, and differ intrinsically from one another in their effects upon this source only in minor characteristics

Each of these electrolytes produces an effect which is best regarded as a variation of the effect of water and varies with the concentration according to a simple law The concentration in each case determines whether the initial value of the demonstrable E M F shall be increased, maintained, or diminished

A study of the comparative effects of various concentrations of the same electrolyte is of particular interest when the nerve is only immersed for a short period (five minutes) presumably because within this period processes of diffusion interfere least with the concentration of electrolytes in the internal parts of the nerve

The concentration law found to unite the effects of solutions of NaCl, KCl, HCl is comparatively simple, the case of solutions of NaOH being apparently more complex

If "E" represents the initial value of the E M F,

"E_n" the value after an immersion of 5 duration in a solution of concentration "*n*,"

"*n*" the concentration in gramme molecules per litre,

then in the special case of solutions of KCl

$$E_n = E \log \frac{1}{n} \quad \text{approximately}$$

Thus, taking the data from four experiments performed upon 5 cm pieces of sciatic nerves (cat), determining the available E M F between cross section and longitudinal surface (*a*) immediately upon removal from the recently killed animal, and (*b*) after an immersion of 5 in a solution of KCl at 17° C —

Experiments with Solutions of KCl

Concentration		E M F recovered, in terms of the initial value E
Grammes per cent	Gramme mols per litre	
7.45	1	$E \times 0.10 = E \log 1.2$
3.72	$\frac{1}{2}$	$E \times 0.34 = E \log 2.2$
1.86	$\frac{1}{4}$	$E \times 0.60 = E \log 4$
0.93	$\frac{1}{8}$	$E \times 0.90 = E \log 8$

The law connecting the effects of solutions of NaCl is not very different, as is shown by examination of the results of the following experiments made before the relation was discovered. The results are in this case none the less remarkable in so far as the concentrations of the solutions used bear no simple proportion to the normal solution of 1 gramme molecule per litre (5.85 per cent)

Experiments with solutions of NaCl

	Concentration		E M F recovered in terms of E the initial value
	Grammes per cent	Gramme mols per litre	
(1)	0.45	$\frac{1}{13}$	$E \times 1.15 \quad E \log 14.1$
(2)	0.6	$\frac{1}{9.7}$	$E \times 0.92 = E \log 8.3$
(3)	0.9	$\frac{1}{6.5}$	$E \times 0.77 = E \log 5.9$
(4)	3.0	$\frac{1}{1.9}$	$E \times 0.34 = E \log 2.2$
(5)	6.0	$\frac{1}{0.97}$	$E \times 0.13 = E \log 1.3$

Presuming temporarily the law connecting these results to be "$E_n = E \log \frac{1}{n}$," then the concentration multiplied by the number of which the logarithm is the proportion between the final and initial value of the E M F, should equal unity

Thus

$$(1) \quad \frac{1}{13} \times 14.1 = 1.09$$

$$(2) \quad \frac{1}{9.7} \times 8.3 = 0.85$$

$$(3) \quad \frac{1}{6.5} \times 5.9 = 0.90$$

$$(4) \quad \frac{1}{1.9} \times 2.2 = 1.15$$

$$(5) \quad \frac{1}{0.97} \times 1.3 = 1.34$$

It is obvious that the law as stated approximately represents the truth in the case of NaCl and KCl solutions. In how far the approximation is different in the two cases must be left to the consideration of further and more exact experiments.

That a similar law is true for solutions of HCl is shown by the following experiments. The stock laboratory solution of 0.4 per cent HCl was diluted twice, &c., to obtain the required variations of concentration.

Experiments with solutions of HCl

	Concentration		E.M.F. recovered in terms of E the initial value
	Grammes per cent	Gramme mol per litre	
(1)	0.2	$\frac{1}{18.2}$	$E \times 0.31 = E \log 2.0$
(2)	0.1	$\frac{1}{36.3}$	$E \times 0.60 = E \log 4$
(3)	0.0250	$\frac{1}{145.2}$	$E \times 1.21 = E \log 16.2$
(4)	0.0125	$\frac{1}{290.4}$	$E \times 1.53 = E \log 33.9$

In this case presuming the statement of the law to be

$$E_n = E \log \frac{k}{n}$$

then

$$\text{in Case (1)} \quad k = \frac{1}{18.2} \times 2.0 = 0.11$$

$$(2) \quad k = \frac{1}{36.3} \times 4.0 = 0.11$$

$$(3) \quad k = \frac{1}{145.2} \times 16.2 = 0.11$$

$$(4) \quad k = \frac{1}{290.4} \times 33.9 = 0.11$$

The law in this case is $E_n = E \log \frac{0.11}{n}$.

It is obvious that in the three cases considered the concentration law is amply expressed as

$$E = k_1 E \log \frac{k}{n},$$

where k_1 and k_2 are constants.

The interest attached to this mode of expressing the law is seen, upon reference to the theory of concentration cells, to be considerable, and the occurrence of the law cannot be otherwise regarded than as a valuable confirmation of the reality of the assumption made as to the nature of the demarcation source of the nerve.

Taking the simplest expression for the value of the E.M.F. of such a cell, in which there are two solutions of different concentrations N and n of the same electrolyte at the same temperature,

$$E = K \log \frac{N}{n}.$$

If in such a cell the solution of concentration "N" is retained constant, whereas the solution of concentration "n" is given different values, n_1, n_2, n_3.. .. &c.,

$$E_{n_1} = K \log \frac{N}{n_1},$$

and

$$E_{n_1} = E \log \frac{N}{n_1} \times \frac{1}{\log N/n}.$$

If N is constant, and also if N/n the original relation of the two solutions is treated as a constant, we have

$$E_{n_1} = E \,.\, k_1 \,.\, \log \frac{k_2}{n_1},$$

and this is the relation found existing between the value of the demarcation source of the nerve before (E) and after (E_{n_1}) the immersion of the nerve in a solution of concentration n_1.

November 15, 1900.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

The Right Hon. Sir Ford North, Professor J. Bretland Farmer, Dr. Patrick Manson, and Professor James Walker were admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair.

Professor J. D. Everett, Dr. J. H. Gladstone, and Dr. R. H. Scott were by ballot elected Auditors of the Treasurer's accounts on the part of the Society.

The following Papers received during the Recess, and published or in course of publication, in accordance with the Standing Orders of Council, were read in title :—

"South African Horse-sickness: its Pathology and Methods of Protective Inoculation." By ALEXANDER EDINGTON, M.B, C.M., F.R.S.E., Director of the Colonial Bacteriological Institute, Cape Colony. Communicated by Sir DAVID GILL, F R.S.

"Note on the Occurrence of a Seed-like Fructification in certain Palæozoic Lycopods." By D. H. SCOTT, M.A., Ph.D., F.R.S., Honorary Keeper of the Jodrell Laboratory, Royal Gardens, Kew.

"The Demarcation Current of Mammalian Nerve. (Preliminary Communication.) Parts I—III." By J. S. MACDONALD, B.A., L.R.C.P.E. Communicated by Professor SHERRINGTON, F R.S.

The following Papers were read :—

I. "Argon and its Companions." By Professor W. RAMSAY, F.R.S., and Dr. M. W. TRAVERS.

II. "Data for the Problem of Evolution in Man. VI.—A First Study of the Correlation of the Human Skull." By Dr. ALICE LEE and Professor K. PEARSON, F.R.S.

III. "Mathematical Contributions to the Theory of Evolution. IX.—On the Principle of Homotyposis and its Relation to Heredity, to the Variability of the Individual, and to that of the Race. Part I.—Homotyposis in the Vegetable Kingdom." By Professor K. PEARSON, F.R.S.

IV. "A Chemical Study of the Phosphoric Acid and Potash Contents of the Wheat Soils of Broadbalk Field, Rothamsted." By Dr. BERNARD DYER. Communicated by Sir J. H. GILBERT, F.R.S.

"Argon and its Companions." By WILLIAM RAMSAY, F.R.S., and MORRIS W. TRAVERS, D.Sc. Received November 13,—Read November 15, 1900.

(Abstract.)

The discovery of krypton and neon was announced to the Royal Society in the early summer of 1898; and subsequently atmospheric air was found to contain a heavier gas to which the name of xenon was applied. Mr. Baly, in the autumn of the same year, called attention to the presence of helium lines in the spectrum of neon, an observation

which confirms that made by Professor Kayser, of Bonn, and by Dr. Friedländer, of Berlin.

At the same time we imagined that we had obtained a gas with a spectrum differing from that of argon and yet of approximately the same density; to this gas we gave the name metargon. It has now been found that the presence of the so-called metargon is to be accounted for by the fact that in removing oxygen from the mixture of these gases, which was then in our hands, phosphorus containing carbon was employed, this mixture when burned in oxygen yields a spectrum to some extent identical with that furnished by carbon monoxide, but differing from it in as much as lines of cyanogen are also present. We have no doubt that the so-called metargon, the spectrum of which is visible only at high pressure, and only when impure phosphorus has been employed to remove oxygen, must be attributed to some carbon compound. In spite of numerous experiments we have not yet succeeded in producing any gas in quantity which yields this composite spectrum. It is only to be obtained by a mixture of carbon monoxide with cyanogen.

To obtain the heavier gases krypton and xenon, a large amount of air was allowed to evaporate quietly; the residue was freed from oxygen and nitrogen, and then consisted of a mixture of krypton, xenon, and argon, the last forming by far the largest portion of the gas; this mixture was liquefied by causing it to flow into a bulb immersed in liquid air, and the bulk of the argon was removed as soon as the temperature rose, the krypton and the xenon being left behind. By many repetitions of this process we were finally successful in separating these three gases from each other. While krypton has a considerable vapour-pressure at the temperature of boiling air, the vapour-pressure of xenon is hardly appreciable, and this afforded a means of finally separating these two gases from one another; in the complete paper the operations necessary to separate them are fully described.

For neon the process of preparation was different. The air liquefier furnished a supply of liquid air; the gas escaping from the liquefier consisted largely of nitrogen; this mixture was liquefied in a bulb immersed in the liquid air which the machine was making. When the bulb had been filled with liquid nitrogen a current of air was blown through the liquid until some of the gas had evaporated. That gas was collected separately, and deprived of oxygen by passage over red-hot copper; it contained the main portion of the neon and the helium present in the air. The remainder of the nitrogen was added to the liquid air used for cooling the bulb in which the nitrogen was condensed. Having obtained a considerable quantity of this light nitrogen it was purified from that gas in the usual manner, and the argon containing helium and neon was liquefied. By fractional distillation it was possible to remove the greater portion of the helium and neon from this mixture of gases, leaving the argon behind. Many attempts were made to separate the helium from the neon. Among these was fractional solution in oxygen, followed by a systematic diffusion of the two gases; but it was not found possible to raise the density of the neon beyond the number 9·16, and its spectrum still showed helium lines. It was not until liquid hydrogen made by an apparatus designed and built by one of us (M. W. T.) had been produced in quantity, that the separation was effected, the neon was liquefied or perhaps solidified at the temperature of boiling hydrogen, while the helium remained gaseous. A few fractionations serve to produce pure neon; we did not attempt to separate the helium in a pure state from this mixture.

That these are all monatomic gases was proved by determination of the ratio of their specific heats by Kundt's method; the physical properties which we have determined are the refractivities, the densities, the compressibilities at two temperatures, and of argon, krypton and xenon the vapour-pressures and the volumes of the liquids at their boiling points.

The results are as follows :--

	Helium.	Neon	Argon	Krypton	Xenon.
Refractivities (Air = 1) ..	0·1238	0·2345	0·968	1·449	2·364
Densities of gases (O = 16)	1·98	9·97	19·96	40·88	64
Boiling-points at 760 mm	?	?	86·9° abs	121·33° abs.	163·9° abs.
Critical temperatures	?	below 68° abs.	155·6° abs.	210·5° abs.	287·7° abs.
Critical pressures	?	?	40·2 metres	41·24 metres	43·5 metres
Vapour-pressure ratio	?	?	0·0850	0·0467	0·0675
Weight of 1 c.c. of liquid..	?	?	1·212 grammes	2·155 grammes	3·52 grammes
Molecular volumes	?	?	32·92	37·84	36·40

The compressibilities of these gases also show interesting features. They were measured at two temperatures—11·2° and 237·3°; the value of P.V. for an ideal and perfect gas at 11·2° is 17,710 metre-cubic-centimetres, and at 237·3° to 31,800. This is, of course, on the assumption that the product remains constant whatever be the variation in pressure. Now with hydrogen at 11·2° C. the product increases with the rise of pressure; with nitrogen, according to Amagat, it first decreases slightly and then increases slightly. With helium the increase is more rapid than with hydrogen; with argon there is first a considerable decrease followed at very high pressures by a gentle increase, although the product does not reach the theoretical value at 100 atmospheres pressure; with krypton the change with rise of pressure is a still more marked decrease, and with xenon the decrease is very sudden. At the higher temperature the results are more difficult to interpret; while nitrogen maintains its nearly constant value for P.V., helium decreases rapidly, then increases, and the same peculiarity is to be remarked with the other gases, although they do not give the product of P.V. coinciding with that calculable by assuming that the increase of P.V. is proportional to the rise of absolute temperature.

These last experiments must be taken as merely preliminary; but they show that further research in this direction would be productive of interesting results.

The spectra of these gases have been accurately measured by Mr. E. C. C. Baly, with a Rowland's grating; the results of his measurements will shortly be published. It may be remarked, however, that the colour of a neon-tube is extremely brilliant and of an orange-pink hue; it resembles nothing so much as a flame; and it is characterised by a multitude of intense orange and yellow lines; that of krypton is pale violet, and that of xenon is sky-blue. The paper contains plates showing the most brilliant lines of the visible spectrum.

That the gases form a series in the periodic table, between that of fluorine and that of sodium is proved by three lines of argument:—

(1) The ratio between their specific heats at constant pressure and constant volume is 1·66.

(2) If the densities be regarded as identical with the atomic weights, as in the case with diatomic gases such as hydrogen, oxygen, and nitrogen, there is no place for these elements in the periodic table. The group of elements which includes them is:—

Hydrogen. 1	Helium. 4	Lithium. 7	Beryllium. 9
Fluorine. 18	Neon. 20	Sodium. 23	Magnesium. 24
Chlorine. 35·5	Argon. 40	Potassium. 39	Calcium. 40
Bromine. 80	Krypton. 82	Rubidium. 85	Strontium. 87
Iodine. 127	Xenon. 128	Cæsium. 133	Barium. 137

(For arguments in favour of placing hydrogen at the head of the fluorine group of elements, see Orme Masson, 'Chem. News,' vol. 73, 1896, p. 283.)

(3) These elements exhibit gradations in properties such as refractive index, atomic volume, melting-point, and boiling-point, which find a fitting place on diagrams showing such periodic relations. Some of these diagrams are reproduced in the original paper. Thus the refractive equivalents are found at the lower apices of the descending curves, the atomic volumes, on the ascending branches, in appropriate positions; and the melting- and boiling-points, like the refractivities, occupy positions at the lower apices.

Although, however, such regularity is to be noticed, similar to that which is found with other elements, we had entertained hopes that the simple nature of the molecules of the inactive gases might have thrown light on the puzzling incongruities of the periodic table. That hope has been disappointed We have not been able to predict accurately any one of the properties of one of these gases from a knowledge of those of the others, an approximate guess is all that can be made. The conundrum of the periodic table has yet to be solved.

'Data for the Problem of Evolution in Man VI—A First Study of the Correlation of the Human Skull" By ALICE LEE, D.Sc, with some assistance from KARL PEARSON, F.R.S., University College, London. Received July 13,—Read November 15, 1900

(Abstract.)

The substance of this paper was a thesis for the London D.Sc. degree; it was shown to Professor Pearson, at whose suggestion considerable modifications were made, and a revision undertaken with a view to publication.

In order to deal exactly with the problem of evolution in man it is necessary to obtain in the first place a quantitative appreciation of the size, variation, and correlation of the chief characters in man for a number of local races. Several studies of this kind have been already undertaken at University College. These fall into two classes, (i) those that deal with a variety of characters in one local race, and (ii) those which study the comparative value of the constants from a variety of races. Thus Dr. E. Warren has dealt with the long bones of the Naqada race,* Mr. Leslie Bramley-Moore has compared the regression equations for the long bones from a considerable number of races in a memoir not

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* 'Phil. Trans.,' B, vol. 189, p. 135.

yet published, Professor Pearson has dealt with the regression equations for stature and long bones as applied to a variety of races;[*] Miss A. Whiteley has studied the correlation of certain joints of the hand,[†] and is investigating the correlation of the bones of the hand in a second local race; Miss C. D. Fawcett has made a long series of measurements on the Naqada skulls, and correlated their chief characters; the present memoir, on the other hand, deals with only a few characters in the skull, comparing, however, the results obtained from a variety of local races.

It is thus related to Miss Fawcett's work much as Mr. Bramley-Moore's to Dr. Warren's, *i.e.*, it endeavours, by selecting a few characters and testing them, to ascertain how far results obtained for one local race are valid for a second. In Professor Pearson's memoir on the reconstruction of the stature of prehistoric races, results obtained from one local race were then extended to a great variety of other races. The degree of accuracy in this procedure can only be fully ascertained when the data now being collected in both English and German anatomical institutes are available for calculation.

The skull, however, differs very widely from the stature and long bones; for, while these have a very high degree of correlation in all local races, the chief characters of the skull are very loosely correlated, and such correlation as they possess varies in a remarkable manner with sex and race. This was first indicated by Professor Pearson;[‡] it has been amply illustrated in the measurements of Miss Fawcett, and is confirmed in a recently published memoir by Dr. Franz Boas. It may be said that this want of correlation in the parts of the skull is the origin of its great importance for the anthropologist; it is the source of its personal and racial individuality. But this anthropological advantage is, from the standpoint of organic evolution, a great disadvantage. Cuvier introduced the conception of correlation with the idea of reconstructing from a single bone the whole skeleton and even the outward form of an extinct animal, but the great want of correlation between the parts of the skull, and between the skull and other parts of the human skeleton, renders quantitative reconstruction—and this is the really scientific reconstruction—of one character of the skull from a second, or of the skull and parts of the skeleton from each other extremely difficult, if not impossible, for all but a very few characters.

Among these characters one of the most feasible to deal with, and one of the most useful, is the capacity of the skull. This is correlated to a fairly high degree (although to nothing like the same extent as the long bones among themselves) with the maximum length and breadth, with the total and auricular heights, and with the horizontal and

[*] 'Phil. Trans.,' A, vol. 192, p. 169.

[†] 'Roy. Soc. Proc.,' vol. 65, p. 126.

[‡] 'Phil. Trans.,' A, vol. 187, p. 279, and 'Roy. Soc. Proc.,' vol. 60, p. 495.

vertical circumferences of the skull. The present memoir deals in the main with the problem of the reconstruction of the capacity from these characters.

Three fundamental problems arise in the theory of reconstruction, *i.e.*, the determination of the probable value of an unknown character from a known and measurable one, or from several such. Namely:—

I. The reconstruction of the individual from data for his own race.

II. The reconstruction of the average value of a character in one local race from data determined for a second local race.

III. The determination of the probable value in an individual of characters not measurable during life from characters which are measurable.

These three problems are all dealt with for the special character capacity of the skull in the present paper. Their importance may be indicated by the following considerations:—

(*a*.) Many, especially of the more ancient and accordingly more interesting skulls, are too fragile or too fragmentary to allow of their capacity being directly determined.

(*b*.) The methods for directly determining capacity are still not only very diverse, but divergent in result, and from the physical standpoint crude and inexact. In the concordat of the German craniologists —the *Frankfurter Verstandigung*—the point was left for future consideration, and so it has remained for many years. The capacities of series of skulls determined during the past forty years in France, England, and Germany are, we are convinced, not comparable, at least if the argument from the comparison is to depend on a difference of 30 to 40 cm.[3] While the same observer using different methods may be trained to get results within 4 to 6 cm.[3] for the same skull, different observers, equally careful, using the same method, will easily get results for the same series diverging by 20 to 30 and even more cubic centimetres. Shortly, the personal equation—involved in the packing in the skull and in the measuring vessel—is very large.

Accordingly a regression equation for the capacity as based on external measurements may, if deduced from a sufficiently large range of series measured by careful independent observers, give results fairly free from the error of personal equation and this sensibly as correct as, or more correct than, direct measurement when we require the mean capacity of a series.

(*c*.) It is impossible to obtain a large series of skulls belonging to known individuals with a classified measure of intellectual ability. Actually we have only a few skulls of men of great intellectual power, sometimes preserved because they were large, and to compare with these the skulls of the unknown and often the ill-nourished, which reach the anatomical institutes.* Accordingly it is an investigation of considerable interest to compare the *probable* capacity of the skulls of living persons with their roughly appreciable intellectual grade. It is only by such a comparison that we can hope to discover whether the size and shape of the skull is to any extent correlated with brain power.

In the course of the memoir it is shown that the auricular height of the skull is a better measurement for determining skull capacity than the total height; that the circumferences of the skull, while highly correlated with its capacity, give regression equations which vary widely from one to another closely-allied race, that linear regression equations involving length, breadth, and auricular height, while giving fairly good results for individuals within the local race, have very divergent coefficients as we pass from local race to local race; that the cephalic index has very little correlation with capacity at all (as a rule what there is may be summed up in the words: In a brachycephalic race the rounder the skull the greater the capacity, in a dolichocephalic race the narrower the skull the greater the capacity—the greater capacity following the emphasis of the racial character); finally, that the correlation of capacity with the triple product of length, breadth, and height gives a regression equation which is fairly constant from local race to local race, and is accordingly the best available.

From this and other equations individual and racial reconstructions are made, and the deviations between the actual and predicted capacities in randomly chosen series of skulls are tabulated. The mean error made in the reconstruction of the individual capacity by the best formulæ is 3 to 4 per cent., the maximum error, although of course infrequent, may even be 10 per cent. For the reconstruction of the mean capacity of a race, the mean error is about 1·2 per cent., with a maximum error of 2·5 per cent. If these errors appear large to the craniologist, we would remind him that his search for an absolutely correct formula giving cranial capacity from external measurements is the pursuit of a Will-o'-the-wisp. The theory of probability shows us exactly the sort of errors such formulæ are liable to, and teaches us how to select the best. The whole basis of the theory of evolution, the variability of one character, even with fixed values for a number of others, would be upset if any such absolute formula were forthcoming. What we have to do is to select a few organs as highly correlated as possible, but, having done this, it has been shown elsewhere that we shall not sensibly decrease the error of our prediction by increasing the number of organs upon which the estimate is based.† Accordingly we do not believe that sensibly better reconstruction formulæ than those found will ever be forthcoming, for, as we have already observed, we know from Miss Fawcett's wide series of skull correlations that we have practically chosen the organs of the highest correlation. Better data for determining the equations will undoubtedly be available as further craniological measurements are made, or as the great mass already made are quantitatively reduced.

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* This argument applies also, in even an intensified degree, to the determinations of brain weight.

† 'Phil. Trans.,' A., vol. 190, p. 466.

In the last place we turn to the third problem: the reconstruction of the capacity of the living head. The memoir contains tables of the skull capacity of some sixty men, and also of some thirty women, whose relative intellectual ability can be more or less roughly appreciated. It would be impossible to assert any marked degree of correlation between the skull capacities of these individuals and the current appreciation of their intellectual capacities. One of the most distinguished of Continental anthropologists has less skull capacity than 50 per cent. of the women students of Bedford College; one of our leading English anatomists than 25 per cent of the same students. There will, of course, be errors in our *probable* determinations, but different methods of appreciation lead to sensibly like results, and although we are dealing with skull *capacity*, and not brain weight, there is, we hold, in our data material enough to cause those to pause who associate relative brain weight either in the individual or the sex with relative intellectual power. The correlation, if it exists, can hardly be large, and the true source of intellectual ability will, we are convinced, have to be sought elsewhere, in the complexity of the convolutions, in the variety and efficiency of the commissures, rather than in mere size or weight.

"Total Eclipse of the Sun, May 28, 1900. Preliminary Account of the Observations made by the Solar Physics Observatory Eclipse Expedition and the Officers and Men of H.M.S. 'Theseus,' at Santa Pola" By Sir NORMAN LOCKYER, K.C.B., F.R.S. Received June 22, 1900.—Read at Joint Meeting of the Royal and Royal Astronomical Societies, June 28, 1900.

The observing station selected for my party was determined upon from information supplied by the Hydrographer, Rear-Admiral Sir W. J. L. Wharton, R.N., K.C.B., F.R.S. Santa Pola appeared likely to meet the requirements of a man-of-war, and without such assistance as a man-of-war can render, the manipulation of long focus prismatic cameras in eclipse observations in a strange country is impracticable.

Santa Pola lies very near the central line of the eclipse, and good anchorage was available, protected from some winds.

Before leaving England, I communicated with Professor Francisco Iñiguez é Iñiguez, Director of the Madrid Observatory, and Mr. Jasper Cumming, H.M. Vice-Consul at Alicante. These gentlemen, together with Don José Bonmati Mas, a large landed proprietor, and father of the Mayor of Santa Pola, very kindly made all the necessary preliminary arrangements with the local authorities, who had also been instructed by the Spanish Government, after representations had been made by the Foreign Office, at the request of the Royal Society.

As a result of the Royal Society's application to the Admiralty, H.M.S. "Theseus," commanded by Captain V. A. Tisdall, R.N., was told off to meet the expedition at Gibraltar, and convey the observers to Santa Pola

The expedition consisted at first of Dr. W. J. S. Lockyer, from the Solar Physics Observatory, Mr. A. Fowler, the demonstrator in Astronomical Physics, from the Royal College of Science, and Mr. Howard Payn, who joined as a volunteer; I subsequently received orders to accompany and take charge of it.

Mr. Payn went on in advance overland to make preliminary arrangements and to lay out the camp on a plan which had been previously arranged, while the remaining observers left England on May 11, by the R.M.S. "Oruba," of the Orient line.

On arriving at Gibraltar, the party at once went on board H.M.S. "Theseus," and left for Santa Pola, which was reached just before noon the following day, May 17. I was glad to find that great interest had been shown in the expedition before our arrival on board, and that lectures on the work to be undertaken had already been given by the Chaplain, the Rev. G. Brooke-Robinson, M.A.

Assistants were at once forthcoming for working the prismatic cameras, and also for manipulating several cameras which I had brought out to be used by the ship's company in obtaining photographs of the corona.

Observing parties in charge of officers of the ship, to make observations along several lines, were at the same time organised.

On our arrival at Santa Pola, the following local officials came on board with Mr. Payn:—Sns. Francisco Bonmati Mas, Mayor of Santa Pola; Antoine Bonmati Mas, Vice-Mayor of Santa Pola; José Bonmati Mas, Municipal Councillor; José Salinas Perez, Municipal Councillor; Eladio Ponce de Leon, Secretary to the Mayor; Michel Sempere, Justice of the Peace; José Hernandez, Captain of the Port; Geronimo Agnati, Administrator of Customs; Eduard Fernandez, 1st Lieut. of Coast Guards; Tomas Bueno, Medical Officer.

They informed us that permission had been given for landing parties from the man-of-war, and special facilities granted for landing instruments and personal baggage without Custom's examination.

The erection of the instruments, huts, and tents was commenced on the following morning, May 18, and by the evening of May 21 the principal instruments were reported in approximate adjustment. Drills were begun on May 22, and were carried on several times a day up to the day of the eclipse. In this work the eclipse clock, which I have described in previous eclipse reports, was used.

By permission of the Captain, three of the officers of the "Theseus," Lieuts. Andrews, Doughty, and Pattrick, R.N., occupied quarters on shore to superintend the work of the parties in the camp. On board, the Chaplain gave instructions in sketching coronas and recording stars, using for this purpose a lantern which had been placed at the disposal of the expedition by the Orient Steam Navigation Company.

The weather was very favourable for the work of the expedition, but at times the landing and embarking of parties from the ship was rendered difficult by strong sea breezes and the consequent surf.

Both day and night the instruments were carefully guarded by a detachment of "Guardias Civiles," told off for the purpose by the Spanish authorities.

The groups of observers were as follows —

LIST OF ECLIPSE PARTIES.

Timekeepers

Lieut. F. A. Andrews, R.N.
Mr. Boughey, Mid
Mr. Lambert, Mid.

J. Walo, 2nd Yeoman Signals.
W. Webb, P.O 1.
Bugler Sneller, O.S

6-inch Prismatic Camera

Dr. Lockyer.
S. Birley, E.R.A.
J. Green, A.B.
C. Fishenden, O S.

C. Willmott, O.S.
A. Humphries, O.S.
G. Hyatt, O S

20-foot Prismatic Camera.

Mr. Fowler.
W. F. Cox, Armr.
A. Whitbourne, A.B.
F. Burt, A.B.

A. Maskell, A.B
E. Davies, O.S.
H. Cristopher, O.S.
W. Harrison, Sto. Mech.

4-inch Equatorial.

Sir Norman Lockyer, K.C.B.

C. C. Lambert, Mid.

3½-inch Equatorial.

Lieut. H. M. Doughty, R.N.

A. G. N. Lane, Mid.

Long-focus Coronagraph.

Mr. Payn.
T. McGowan, A.B.
E. Woodland, A.B.

H. Eary, A.B.
W. Mann, O.S.
H. Brooks, O.S.

Graham Coronagraph.

Mr. W. J. S. Perkins, Asst. Engr., R.N.
W. Walker, Lg. Stoker.
J. Knowles, Chief Stoker.

De La Rue Coronagraph

Mr H. W. Portch, Asst. Engr, R.N.
W. Waterfield, E R.A.
H. Frost, Chief Stoker

Dallmeyer Coronagraph.

Surgeon J. Martin, R.N.
E. Buckingham, E R.A.
R. Quint, Chief Stoker

Discs.

Mr. J. B. Bateman, Mid. R.N.
W. Fraser, Arm. Crew
R. S. Bradbrooke, A.B
Mr. J. A Daniels, Torp Gunner, R.N.
G. Fair, Armourer
E. Gordon, S. Carp.

H. W. Richardson, P.O. 2.
E. Voyle, Lg. Shipwt.
T. Orange, Boy, 1 c.
W. Tucker, A B
W. Brewer, A B
B. Salmon, Boy, 1 c.

A. Mason, A.B.
A. Steven, A B.
C. Paul, Boy, 1 c.
A. May, A B.
H Bailey, A B
J. Entwistle, S Std Boy

Sketches of Corona without Discs (on shore).

W. Butt, M.A.A.
G. Guilliame, A.B.
H. Meacher, Pte. R.M L.I.
H. Schmidtœl, O.S.

Sketches of Corona without Discs (on board).

W. Baxter, A.B.
W. Butts, Pte. R.M.L.I.
C. Jacob, Pte. R.M.L I.
J. Wheeler, Pte. R M.L I.
E. Willis, S.B. Attendant.

Observations on Stars (on shore)

Mr Bennett, Clerk.
W. Riches, L. Seaman.
A. Pontifex, A.B.
W. Bosworth, A.B.
H. Angus, O.S.
W. Kinvett, Pte. R.M L.I.
W. Oliver, Pte. R.M L I.

Observations on Stars (on board).

Rev. G. B. Robinson, M.A.
H. Croxon, N. Corpl.
A. Phillips, Leading Shipwt.
R. Vigus, Corpl. R.M.L.I.
E. Price, Pte. R.M.L.I.
E. Hammond, Sto
G. Andrews, Sto
G. Nightingale, Sto.
S. Wilson, Sto
E. Savage, Pte. R.M L I.

Observations of Shadow Bands (on shore).

Commander Hon. R. F. Boyle, R.N.
Mr. T. Slator, Naval Instructor, R.N.
Mr. J. G. Walsh, Mid. R.N.
Mr. F. C. Skinner, Mid. R.N.

Meteorological Observations (on shore).

Lieut. Pattrick, R.N.
Mr. G. S. Hallowes, Mid. R.N.

Meteorological Observations (on board).

G. Donnelly, Yeom Sig.
E. Gant, Lg. Sig
A. Enstidge, Sig
W. Hearne, Sig.
J. Beach, Sig.

Landscape Colours (on shore).

Capt. F V Whitmarsh, R M.L.I
Ship's Steward D. Green.
Lance-Corpl. Wade, R.M.L.I
W. Birkett, Writer.

Landscape Colours (on board).

Fleet Paymaster A. W Askham, R.N
Lieut W. J. Frazer, R N.

Shadow Phenomena (on shore)

Mr C Prynn, Carpr. R.N

Shadow Phenomena (on board).

Lieut. H R. Shipster, R N

Photographers

J Knight, S B Steward.
B Bulbrook, A.B.

Aide-de-Camp to Sir Norman Lockyer, K.C.B., F.R S.

Mr. C. C Lambert, Mid R N.

Time Arrangements.

According to the Admiralty chart, the latitude and longitude of the place of observation are 38° 11′ 20″ N. and 0° 33·66′ W. respectively. For this point, the times and position angles of contact derived from the formulæ given in the 'Nautical Almanac Circular,' No. 17, were as follows :—

Beginning of totality, May, 28 d. 4 h. 12 m., 51·7 s.
End " " 4 h. 14 m., 10·5 s.
Duration of totality .... 1 m., 18·8 s.
Position angle of first contact, 87° 3·5′ from N. towards W.
" " last " 93° 47·3′ " " E.

The experience of the Indian eclipse of 1898 suggested that the duration of totality was too long, and for the practical working during the eclipse the adopted time was 75 seconds, so that there would be no chance of spoiling the coronagraph plates by exposing them after totality. The face of the eclipse clock was graduated accordingly.

The arrangements for securing signals at definite intervals before totality was identical with that employed in Lapland and India. An image of the sun projected by the finder of the 6-inch two-prism prismatic camera was viewed on an adjustable screen, marked in such a way that it was easy to see when the cusps subtended angles of 90°

and 55°, which occurred respectively at 16 secs. and 5 secs. before totality. The signals "Go" at the commencement of totality, and "Over" at the end, were given by myself, from observations made with the 4-inch Cooke telescope.

Results.

Some of the photographs have not yet been developed, and the reports have not yet been received from the ship's parties, so that only a very brief reference to the work accomplished is possible.

The Prismatic Cameras.

The discussion of the series of photographs taken with the prismatic cameras employed in the last three eclipses indicated that continued work with this form of spectroscope should be undertaken, (1) with the view of obtaining data strictly comparable with the previous photographs, and (2) that an effort should be made to extend the inquiry into comparative lengths of the various arcs. For the first purpose it seemed desirable to repeat the Indian work with the 6-inch camera having two prisms, while for the second an instrument of longer focus was necessary.

Representations as to the importance of the latter instrument were made to the Royal Society, and ultimately the purchase of a Taylor triple lens of 6 inches aperture and 20 feet focal length was authorised. This was received so shortly before the expedition left England, that it was only possible to make a rough trial of the instrument before it was set up at Santa Pola. Both prismatic cameras were worked in conjunction with siderostats, calculations having shown that the position angles of contact were favourably situated after reflection.

Dr. Lockyer took charge of the two-prism instrument, and Mr. Fowler of that having a long focus, and in each case the programme of exposures was successfully performed.

The photographs which have been developed indicate the same succession of phenomena recorded in the three previous eclipses, but the recent eclipse was specially advantageous, for the reason that the chromospheric arcs at the instant of contact were of greater length.

A very complete record of the spectrum of the chromosphere at various depths has been secured with both instruments, and it seems probable that new information as to the distribution of the various vapours will be furnished by the photographs taken with the long-focus instrument.

The spectrum of the corona shows the green ring at 5303·7, the blue ring at 4231, and the violet ring at 3987·0: others may possibly appear on closer examination. All the rings are of totally different character from the chromospheric arcs, and have their greatest brightness in regions other than those where the chromospheric arcs are brightest. As before, 3987·0 is much more uniform in brightness throughout the extent of the ring than the others; 5303·7 is especially strong in one or two regions, but on the whole is probably weaker than in 1898.

The photographs show that the scale of the spectra is by no means too large for work with short exposures with a lens of 6 inches aperture. The spectra are 7·5 inches long from D_3 to K, and the diameter of the rings is 2·5 inches; photographs taken with an exposure estimated at $\frac{1}{4}$ of a second are fully exposed.

The Differences between the Coronas observed at the Periods of Sun-spot Maxima and Minima.

My attention was called especially to these differences, because I saw the minimum eclipse of 1878, while the phenomena of that of 1871 (maximum) were still quite fresh in my mind. My then published statements have been amply confirmed during the eclipses which have happened since 1878, but certainly the strongest confirmation has been obtained during the present one, which took place two more spot periods after 1878.

1. *Form.*

With regard to form, at the instant of totality I saw the 1878 corona over again, the wind vane appearance being as then most striking.

2. *The Spectrum.*

In connection with the eclipse of 1878 (minimum), I pointed out that, whereas in 1871 (maximum) the spectrum of the corona viewed by small dispersion was remarkable for the brightness of the lines; in 1878 they were practically absent, and the continuous spectrum was remarkably brilliant.

I determined therefore to make a similar observation in this year of maximum, and, as in 1878, used a grating first order spectrum placed near the eye. The result was identical with that recorded in 1878. I saw no obvious rings or arcs, but chiefly a bright continuous spectrum.

3. *The Minute Structure of the Inner Corona.*

Lieut. Doughty, R.N., and myself made observations on the minute structure of the corona, in order to see if any small details could be observed, and whether they were the same as those I saw so well and recorded during the eclipse of 1871, at a period of sun-spot maximum. This question was specially taken up this year, as exactly two sun-spot periods have elapsed since 1878.

In 1871 I used a 6-inch object glass, and distinctly observed marked delicate thread-like filaments, reminding one of the structure of the prominences, with mottling and nebulous indications here and there; some of these distinct markings were obvious enough to be seen till some minutes after totality.*

This year, with a perfect 4-inch Taylor lens and a high power, not the slightest appearance of this structure was to be traced; the corona some 2′ or 3′ above the chromosphere was absolutely without any detailed markings whatever.

Lieut. Doughty duplicated and confirmed these observations with a 3¾ Cooke. Here, then, is established another well-marked difference between maximum and minimum coronas

The Coronagraphs.

Four coronagraphs were employed of various apertures and focal lengths. One, of 4 inches aperture and 16 feet focal length, was in charge of Mr. Howard Payn, while the others were controlled by officers of the ship.

The results obtained are very satisfactory, those taken with the long-focus instrument being especially good. In this case the image is 1¾ inches in diameter, and the definition is perfect. The photograph taken with an exposure of 5 seconds shows a great wealth of detail in the inner corona and prominences; the fine definition appears to be due to the fact that a Taylor photo-visual lens was employed, bringing the rays of various refrangibilities to the same focus. A long exposure photograph, with the same instrument, is remarkable for the perfect hardness of the moon's edge, notwithstanding the motion during totality.

The three photographs secured by Asst. Engineer Portch, R.N., with the De la Rue lens of 4⅝ *inches* aperture, give also sharp images with much fine detail.

Sandell triple-coated plates were used with this instrument.

With the 6-inch Dallmeyer lens, two photographs on Sandell plates were obtained by Dr. Martin, R.N., one being exposed for about half a second, and another for 50 seconds.

The longer exposure records the extensions to a greater distance from the dark moon than any of the other photographs obtained, with the exception of the one secured with the small-grating camera.

This last-mentioned instrument consisted of a Zeiss anastigmatic lens of 9 inches focal length, with a small Thorp grating mounted in front of it. The exposure of the plate was 40 seconds during totality; the longest streamer in the N.E. quadrant extends to a distance of 4½ lunar diameters.

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* 'Solar Physics,' p. 372.

Discs.

Six discs for cutting out the bright light of the inner corona were erected, with the view of enabling the observers to detect the long extensions if there should be any. They were very carefully set up by Lieuts. Doughty and Andrews, R.N., and were provided with eyepieces having all necessary adjustments. Mr. Daniels, torpedo gunner, then took charge of the party, and numerous rehearsals were given. In the trials remarkable skill in recording delicate details was displayed.

During the eclipse, the actual observer was blindfolded for five minutes before totality.

No extensions of the nature observed by Professor Newcomb in 1878 were recorded.

Observations on the Stars Visible during Totality.

A large party for the observation of stars visible during totality was trained and organised by the Chaplain, Rev. G. Brooke-Robinson, R.N., who was provided with a set of star charts for purposes of instruction prior to the eclipse, and another set, prepared by Dr. Lockyer, for making records during the eclipse.

Venus became visible at a very early stage of the eclipse, and during totality Mercury was a very conspicuous object near the extremity of one of the streamers. α Tauri, α and γ Orionis were also recorded. No comet or unknown body was noted.

Shadow Bands.

The Naval Instructor on H.M.S. "Theseus," Mr. T. Slator, B.A., undertook this branch of the eclipse work, and during the eclipse worked in conjunction with the Commander, the Hon. R. F. Boyle. Very complete arrangements were made for securing the orientation of the bands (1) on a horizontal plane; (2) on a plane in the meridian; (3) on a plane in the prime vertical. The bands appear to have been very ill-defined, but the necessary observations were secured in planes 1 and 2.

Meteorological Observations.

A regular series of observations of temperature and pressure was established three days before the eclipse, and continued until two days after; Lieut. Pattrick, R.N., taking charge of this branch of the work. During the eclipse the temperature fell 5° C., and the barometer also fell slightly.

The thanks of the expedition are due especially to those named in the foregoing account, not only for assistance rendered, but also for their great kindness to us. I have already, in a letter, expressed to the Royal Society my deep sense of the obligation they have laid us under.

As in the case of the "Volage" and "Melpomene," the officers and men of the "Theseus" not only assisted us with certain instruments, but organised crews for others, and many lines of work which it was impossible for the observers sent out from England to attempt. Their skill, resourcefulness, and steadiness were alike truly admirable.

Thanks are also due to the Managers of the Orient Steam Navigation Company, who conveyed the instruments to and from Gibraltar freight free

I may add, the Civil Governor of the Province of Alicante, Señor don Hipoldo Caras y Gomez de Andino, visited the camp to assure himself that all the assistance the Spanish authorities could give had been rendered.

"Total Solar Eclipse of 1900 (May 28). Preliminary Report on the Observations made at Bouzareah (in the Grounds of the Algiers Observatory)."

By Professor H. H. Turner, M.A., F.R.S., and H. F. Newall, M.A., Sec. R.A.S.

Received June 28,—Read at Joint Meeting of the Royal and Royal Astronomical Societies, June 28, 1900.

The Report is presented in three parts.

Part I. Origin of the Expedition and General Preparations by the Two Observers jointly (§§ 1—10).

Part II. Separate Report by Professor Turner.

- §§ 11—12. The Cameras and Cœlostat.
- § 13. The Polariscopes.
- §§ 14—16. Adjustments.
- §§ 17—19. Programme of Observations.
- §§ 20. The Standard Squares.
- § 21. Use of Green Screen.
- § 22. Integral Photometer.
- § 23. Development.

Part III. Separate Report by Mr. Newall.

- § 24. The Four-prism Spectroscope with Slit.
- § 25. The Photographic Camera with large Objective Grating.
- § 26. The Polariscopic Camera (Savart Plates and Nicol Prism).
- § 27. Atmospheric Polarisation.
- § 28. General Observations.

PART I.

1. *Origin of the Expedition.*—This expedition was one of those organised by the Joint Permanent Eclipse Committee of the Royal Society and Royal Astronomical Society, funds being provided from a grant made by the Government Grant Committee.

The expedition was most cordially and hospitably assisted by M. Trépied, the Director of the Algiers Observatory, and the observers are indebted to him in numberless ways for his kindness. He assigned good positions for the instruments in the Observatory grounds, and had brick piers built beforehand according to plans supplied to him by the observers. He made the arrangements for conveying the instruments to and from Algiers, and put at the disposal of the observers a capacious dark room (which we believe he had specially arranged for the purpose) and the services of a carpenter.

2. *Mr. Wesley's Observations.*—It may be here mentioned, although it does not come strictly within the scope of this report, that M. Trépied allowed Mr W. H. Wesley, the Assistant Secretary of the Royal Astronomical Society, who has had great experience in drawing the corona from photographs, to use the equatorial coudé of the Algiers Observatory during this eclipse; and Mr. Wesley was thus enabled to make his first eye observations on the corona itself under most favourable conditions. He joined the present expedition, but as he was the emissary of the Royal Astronomical Society and not of the Joint Committee, the report of his observations is not included here. That M. Trépied should have placed the finest instrument in the Observatory at the disposal of a foreigner is a striking instance of his scientific liberality; and the observers call attention to it because it will indicate more clearly than any enumeration of details the kind of assistance for which they have to thank him.

3. *Personnel.*—The following persons took part in the expedition:—

H. H. Turner, M.A., F.R.S., Savilian Professor of Astronomy at Oxford.

H. F. Newall, M.A., Sec. R.A.S., Observatory, University of Cambridge.

4. *Itinerary.*—The observers left Charing Cross at 11 A.M. on Saturday, May 12. They spent one day in Marseilles, and arrived at Algiers on Tuesday, May 15, proceeding in the evening of the same day to the little village of Bouzareah, which they made their headquarters, about a mile from the Algiers Observatory. The instruments had been sent round by sea (through the Papayanni Steamship Company), and should have arrived on May 10, but for some reason they did not arrive until May 17, and were delivered at the Observatory on the evening of May 18. Three whole working days of the eleven which had been

counted on were thus lost, and in order to carry out the programme an undesirably great press of work was necessary. The day of the eclipse was fine, and many good photographs were obtained. The development of these and the packing up of the instruments fully occupied the observers till Friday, June 1. They left Algiers on Saturday, June 2, and arrived in London on Monday, June 4. But they would record the opinion that the time spent on the expedition was too short. The work was got through, but with practically no margin for contingencies, and would have been done better with another week at least.

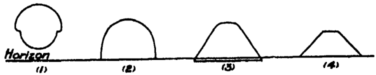
5 *Position of Station.*—The station was on the west side of the equatorial coudé, and about 50 yards S.E. of the transit-circle, the position of which is

Longitude . $0^h 12^m 8^s{\cdot}7$ E of Greenwich.
Latitude . . . . . . 36 48′ 0″ 5 N
Height above mean sea level, 1123 feet.

This spot was some distance from the central line, and 4 or 5 seconds of the 70 seconds of totality available were thus lost; but the loss was more than counterbalanced by the many advantages of being at a fixed observatory.

6 *Meteorological Conditions.*—As regular meteorological observations were made at the Observatory, none were made by us. The day of the eclipse was the finest of our stay, and fine days preceded and followed it. On May 26, 27, and 28 the sun was seen to set in the sea, and the "green ray" was looked for and seen by several observers.

The disc, when near the horizon on May 28, assumed remarkable shapes, of which the following four types were noticed by several observers:—



There was at times considerable wind, as M. Trépied had warned us, but the day of the eclipse was calm.

7. *Instruments, &c.*—(See separate reports of observers.)

8. *Huts.*—Willesden canvas over wooden framework was used, and found very satisfactory, as before.

Mr. Newall's hut was designed for his particular instruments, and the openings were obtained by leaving the canvas loose in the form of flaps, which were tied in the proper positions, either open or closed.

Professor Turner's hut was designed for general requirements, and has now been used, not only in this expedition, but as a transit hut in the determination of the longitude of Killorglin by the staff of the Royal Observatory, Greenwich, in 1898. As it appears to satisfy the conditions, the following notes of its structure may be useful to others:—

It is a skeleton wooden framework filled in by a series of panels, any one of which is removable without disturbing any other by simply taking out two screws. The panels forming the sides drop into a groove running round the base, and two screws are sufficient to hold them at the top. For the roof panels it is the upper edges which push into grooves along the central ridge, and the two fixing screws are near the eaves.

The panels themselves are rectangular wooden frames with canvas stretched over them. For transport, the sides are unscrewed, and then the canvas is rolled round the ends like a window blind.

The screws which fix the panels in position in the hut terminate in rings instead of the ordinary screw heads, so that they can be screwed up or unscrewed with the fingers instead of with a screw-driver, which may not be handy at the moment.

It may be remarked that both the huts were securely fastened down on this particular occasion, as the wind sometimes blew a gale.

9. *Assistance*.—The observers were assisted in the exposures as follows:—

Mr. H. Wyles, of the Leeds Astronomical Society, counted seconds aloud from a metronome.

Mr. J. Potter, of Leeds, carried from Mr. Newall's hut the information of the setting of the Savart prism (which Mr. Newall was to observe during totality) to Major K. O. Foster, who set the corresponding instrument in Professor Turner's hut (see separate report of Mr. Newall). It was originally intended to shout this information, but as it was found in the rehearsals that there was occasionally difficulty in hearing, Mr. Potter undertook this conveyance as a safeguard. As the event proved, his assistance was all important, for at the actual eclipse there was so much noise from other observers in the neighbourhood that the shout was not heard at all.

Major K. O. Foster, F.R.A.S., set Mr. Newall's savart between the second and third exposures, and at the same time changed the slit of Professor Turner's polariscope. He also uncovered the plates for long exposure soon after the beginning of totality and covered them before the end.

Mr. F. L. Lucas, of Berkhamsted, made the exposures for Professor Turner at the objective.

Master Eric Henn handed the plates.

Mr. F. L. Crawford, of the Indian Civil Service (who had seen the

1898 eclipse at Berar), received the plates, recorded the times, and also exposed for 10 seconds the integral photometer.

Mr. Lovett Henn, of Algiers, made the exposures with the grating for Mr. Newall.

Mrs. Newall made observations of the atmospheric polarisation during totality.

At 15 seconds before totality, as shown by the diminishing crescent of the sun, Professor Turner called "Stand by"; at totality, "Start": when Mr. Wyles counted from the metronome steadily up to 80. Totality lasted 64 or 65 seconds, and the extra 15 seconds was required by Mr. Newall for exposures at the second "flash." The signals were given with approximate correctness, though, by an oversight, no one timed the interval between the "Stand by" and the "Start."

The operations were rehearsed several times on the day before the eclipse, and once or twice in dumb show on the actual day. It was not found possible to arrange for rehearsals earlier; but, with the exception of the omission just noticed, everything went off at the time without a hitch.

10. *The Day of the Eclipse.*—Perfectly clear all day—no anxiety. The contacts were not observed by us with special care as we had much else to do, and observations were being made by the staff of the Observatory. M. Sy kindly supplied the following predictions and observations:—

	Predictions.	Observations.
1st contact ..	$3^h\ 17^m\ 21^s$	$3^h\ 17^m\ 18^s$
2nd ,, ...	4 29 25	4 29 27
3rd ,, ...	4 30 32	4 30 32
4th ,, . ..	5 34 31	5 34 25

Local mean time ($12^m\ 8^s{\cdot}7$ in advance of G.M.T.).

Lamps were not needed during totality.

Owing to an accident (a signal being lost through noise made by others) the shadow was not observed. Major Kingsley Foster noticed the "shadow bands" on the white surface of the "double tube" near which he was stationed.

PART II.—SEPARATE REPORT BY PROFESSOR TURNER.

Instrumental Equipment.

11. *The Cameras.*—The double camera used at Fundium in 1893 (by Sergeant Kearney), and at Sahdol in 1898, was modified on the present occasion. One of the 7 × 7-inch tubes contained, as before, the photoheliograph objective No. 2 of 4-inch aperture and 5 feet focal length, with a Dallmeyer secondary magnifier of 7½ inches focus placed

5 inches within the focus, giving an image of the sun $1\frac{7}{8}$ inches in diameter; but the "Abney" lens was no longer used in the other tube. It had been decided by the Joint Permanent Committee to discontinue the separate use of the two Abney lenses, and to recombine them into the original doublet, which Mr. Davidson was to use in the expedition organised by the Astronomer Royal. Hence the other half of the double tube camera was set free, and it was utilised to good effect by arranging *two* polariscopic cameras to give images on the same plate, a diagonal partition dividing the square tube into two. One instrument was arranged by Mr. Newall, and is described by him. The other was similar to the apparatus used by me in India in 1898, but with improvements in detail as described below The double camera is furnished with six plate holders, each taking two plates of 160 × 160 mm. (as in use for the Astrographic Chart), both plates being exposed by a quarter turn of one shutter.

Alongside the double tube two other cameras were arranged for single exposures during the greater part of totality. One was a portrait lens of $5\frac{1}{4}$ inches aperture and 30 inches focus, stopped down to $f/8$; the other was a small polariscopic camera, described below

12. *The Cœlostat.*—All these cameras were pointed downwards at an angle of 18° with the horizon, in azimuth 42 west of south, to the 16-inch cœlostat used in India in 1898. The mirror of this instrument was made by Dr. Common. It was silvered and sent out to Algiers by the Improved Electric Glow Lamp Company, and had a very fine surface. The mounting and clock of the instrument were made by Mr. J. Hammersley, from designs by Dr. Common. A steadier mounting is desirable on future occasions, though the present arrangement works well when there is not much wind to cause vibration.

13. *The Polariscopes.*—The arrangement used in India was as follows:—

(A) Objective, $3\frac{1}{2}$ inches aperture, 18 inches focus.
(B) Slit, of width 0·2 inch, in cardboard.
(C) Collimator, $1\frac{1}{2}$ inches aperture, $6\frac{1}{2}$ inches focus.
(D) Rhomb of spar, 1 inch aperture (clear).
(E) Camera, 2 inches aperture, 9 inches focus.

On the present occasion (E) was substituted for (A), which was of inconvenient width for the space at disposal. The primary image was thus reduced to half the size; but this had the advantage that a larger part of the image fell on the slit, the width of which remained the same as before, being governed by the focal length of the collimator and the angular separation of the images by the rhomb. The collimator (C) and the rhomb (D) remained unchanged, but the camera lens (E) was now a photographic objective of $1\frac{1}{2}$ inches aperture and 28 inches focus, made specially by Messrs. Cooke and Sons, of York.

The plate holder was of course that of the double tube, as above explained.

The slit (B) was arranged, as in 1898, in two portions, but was on this occasion made in brass.

The slit and rhomb were connected by a bar, and could be rotated sympathetically. They were set at such a position angle that the lines of the image parallel to the slit corresponded to vertical lines on the corona; but this setting was found after the eclipse to be not quite accurate. The setting was not changed during totality, but the slit was moved in the direction of its length, so as to give a different part of the field between the second and third exposures.

The small polariscope exposed separately resembles the objective prism spectroscope as opposed to the slit spectroscope. The reason for adopting the slit spectroscope form for the instrument above described is that the angular separation of images given by the large rhomb was not large, and if this rhomb had been simply placed in front of an objective, one image of the corona would have seriously overlapped the other. But a small rhomb (kindly lent me by Mr. Newall) gave a separation of 3½°, so that when the corona was viewed through this rhomb and an objective the two images polarised in perpendicular planes were clearly separated, though each was projected on the sky of the other. To cut out the sky backgrounds, a slit, of 1 inch aperture, was placed 15·7 inches in front of the rhomb.

Instrumental Adjustments.

14. *Adjustment of Cœlostat.*—The adjustment of the polar axis was made as described in the Report on the Japan Expedition,* by means of the attached declination theodolite. This was a new one, by Messrs. Troughton and Simms, of rather smaller size than the others, with a 3-inch circle reading to 1′ only. When the three eclipse cœlostats were constructed, theodolites were only supplied with two of them; and as in 1896 and 1898 there were two cœlostats at the same station, one theodolite sufficed for adjusting the two. But this arrangement was only provisional, and on the present occasion, when all three cœlostats went to different stations, it became necessary to provide the third theodolite. From previous experience I judged that the smaller size would be sufficient for the purpose.

The following observations will sufficiently indicate the state of adjustment, those with the level being made on the meridian and compared with the known latitude, so as to give the same sign to the errors as the sun observations:—

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* 'Monthly Notices, R.A.S.,' vol. 57, p. 102.

Date.	Sun's H.A.	Obs. decl.	Tab decl.	O—C.
May 22	− 3 0	+ 20° 17′	+ 20° 20′	− 3′
		Observation with level		− 1
May 29	− 1·0	+ 21° 36′	+ 21 35′	+ 1′
		Observation with level		− 1′

On the present occasion the cœlostat was not blocked with wood as in India; but it was found liable to slight vibration, and a firmer support should be provided for future eclipses.

15. *Tilt of Mirror.*—The time available for preparation was so fully occupied that no special observations for tilt of the mirror were made, but in India there was found to be no appreciable tilt.

16. *Focussing of Telescopes.*—The method adopted for the photoheliograph objective and magnifier was that described in the Report on the Japan Expedition.* The position found was very close to that found in previous expeditions. The object glass was unscrewed a quarter turn, *i.e.*, 0·02 inch, differing 0 02 inch from the position in 1898, as indicated by a wooden gauge

The polariscopes were focussed on a distant view (a clear view of 7 or 8 miles across the bay was on this occasion available) through blue glass

The portrait lens was first focussed by blue glass, and then three photographs of the distant view were obtained, which gave the focus sharply.

A focussing eye-piece made in blue glass, kindly lent by M. Trépied, was found very useful. I do not know whether this simple and convenient apparatus is well known, it is made by Hermagis, of Paris.

17. *Programme of Observations with Double Tube*.—The six slides of the double tube camera were filled with "Rocket" plates; four of them only were to be used during totality, the others being available for supplementary short exposures if time should allow. As it turned out, there was no time for more than the four, and the others were exposed just after totality. The actual exposures were as below:-

No of slide.	Exposure in seconds.	Time in beats from commencement of totality.	Time in seconds.
1	1	6—7	5—6
2	5	13 - 18	12—17
(Setting of Savart and slit between these.)			
3	19	31—51	29—48
4	5	55—60	52—57
End of totality		66	62
5	1	68—69	64—65
6	1	75—76	71—72

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* 'Monthly Notices, R.A.S.,' vol. 57, p. 105.

The times after totality are diminished to get true seconds. The numbers in the third column are beats of a metronome, which, though adjusted beforehand, changed its rate, perhaps owing to the fall of temperature. Just after totality it was found to beat sixty-three times to the minute. Further, the word "One" was called on an actual beat which came about 0·5 second after the word "Start" (signifying the commencement of totality) had been called. This signal was given by me from a direct observation of the disappearance of the crescent, and agreed well with the observations of others. The signal for 15 seconds before totality was given to Mr. Newall by watching the length of the disappearing crescent on the focussing glass of the camera, and was approximately correct, though by an oversight no one observed the interval; but after giving this signal, as I found the direct light of the crescent did not hurt the eyes, I watched that in preference to the image on the glass. I saw the complete ring of the moon's disc quite 10 seconds before totality, and from that moment the corona seemed to grow out from the limb in a most beautiful manner.

18. *Programme for the Polariscopes.*—The two polariscopes mounted in the double tube had of course exactly the same exposures as above.

Between exposures of slides 2 and 3 the Nicol prism and Savart plate of Mr. Newall's polariscopic apparatus were rotated by Major K. O. Foster to the reading indicated by Mr. Newall's eye observations; and the slit of my apparatus was also moved by Major Foster to the second position, so that the second pair of photographs gave a different part of the corona from the first.

The smaller polariscope was exposed from 5 to 60 seconds, counting from the beginning of totality.

19. *Programme for the Portrait Lens.*—A Sandell triple-coated plate was exposed in this instrument. The exposure was made by Major Foster from 5 to 60 seconds, counting from the beginning of totality.

20. *The Standard Squares.*—On the six plates in slides, 1, 2, and 3, Sir W. Abney's "standard squares" were impressed for photometric observations of the corona. The exposures were to a standard candle at 5 feet from the plate, which is approximately twice as bright as the full moon. Assuming the brightest part of the corona to be as bright as the average surface of the full moon, the exposures to be given to the candle were calculated as follows:—

An image of the moon in the Dallmeyer lens of 4 inches aperture would be $1\frac{1}{2}$ inches in diameter. The illumination of the object glass is thus concentrated—

$$(4/1\tfrac{1}{2})^2 \text{ times} = 7 \text{ times.}$$

Hence the brightest part of the corona will affect the plate about seven times as much as direct moonlight, or three and a half times as much as a candle at 5 feet.

Hence to compare with a 1 second exposure to the corona through the lens, we should expose the plate to the candle for 3⅓ seconds. Since the faintest of the standard squares obstructs some of the light, and since it is advisable to have an exposure on the plate from the standard light rather denser than the densest part of the image, the plates in slide No. 1 (to be exposed 1 second to the corona) were exposed for 6 seconds to the candle.

Those in No. 2 (to be exposed 5 seconds to the corona) were exposed for 40 seconds to the candle, and those in No. 3 (20 seconds to corona) for 2½ minutes. These exposures are even longer, relatively, than No. 1; but on previous occasions the squares had not been dense enough, and it was considered advisable to make sure of going beyond the point required.

21. *Use of Green Screen.*—In slide No. 4 a coloured glass screen, kindly provided by Mr. Shackleton, was placed in front of the plate, with a view of obtaining information on the distribution of coronium in the corona by comparing a photograph taken in green light with the others. But owing to the following circumstance this particular experiment was not a success. The plate should of course have been one sensitive to green light, and some Cadett "spectrum" plates were taken out to Algiers for the purpose. They were whole plates and required cutting down to fit the slide. In the stress of other work the experiment was forgotten until an hour before totality There was still plenty of time to fill the slide, and I went to the dark room to do so. But the circumstances were scarcely favourable for manipulating the diamond in the dark. as these plates require. The first plate broke and cut my finger, not seriously, but enough to hamper me, so that I had no better success in cutting another plate. Indeed, after one or two attempts I had to give it up. It seemed just worth while putting in a "Rocket" plate behind the green screen, but there was very little on the plate when developed, and the experiment was on this occasion of little or no value. I am sorry to have been unable to do justice to Mr. Shackleton's kindness in providing the screens, and hope that on another occasion I may make better use of them.

22. *An Integral Photometer.*—To obtain an estimate of the total light given by the corona, an Ilford "Empress" plate was exposed to its light for 10 seconds, in a small camera from which the lens had been removed, so that the corona shone directly on the plate, but side light was excluded. On the same plate standard squares were impressed by exposing it to the candle at 5 feet, as in § 8. The exposure given to the candle was, by an oversight, not recorded, but was either 10 or 20 seconds. The oversight was discovered before the eclipse, and another plate from the same batch was exposed to the candle and squares for 5, 10, and 20 seconds, and developed in the same dish. The effect of the corona was, however, considerably greater than that

of the 20 seconds' exposure through the thinnest of the square screens.

23 *Development*.—The plates in slide No. 1, and the corona picture of slide No. 2 (1 second), were developed with amidol; also the plate exposed to the integral photometer mentioned in § 12. All the others with metol.

PART III.—SEPARATE REPORT BY H. F. NEWALL.

§ 24. *The Four-prism Spectroscope with Slit.*

It was intended to attempt—

(i) To secure photographs of the bright-line spectrum of the sun's limb at the beginning and end of totality, five photographs at the beginning, six at the end.

(ii) To photograph the spectrum of the corona in two separate regions of the corona.

It had been decided not to attempt to determine the velocity of rotation of the corona, for the duration of totality was not long enough to give satisfactory images of the lines in the spectrum of the corona at such distances from the limb as would ensure some measure of certainty that the observations would not deal with the local disturbances known to exist near the chromosphere.

The instrument arranged for the purposes above mentioned is a four-prism spectroscope with a single slit. It was used by the writer in India, at Pulgaon in 1898.* The only changes made in it were that (i) only one slit was used instead of two; and (ii) one of the prisms which had been found to give imperfect definition on account of want of homogeneity in the glass had been replaced by another prism. The prism box and train of prisms had been used at the Cambridge Observatory for a star spectrograph, and were dismounted, for use in the eclipse, after the completion of certain observations of Capella.

The train of prisms is of such dimensions and construction as to transmit a 2-inch beam of light, and to produce a minimum deviation of 180° for H_γ. The collimator and camera are set parallel to one another.

The whole spectroscope is mounted so as to turn about an axis parallel to the collimator. The axis is rotated (with a period of twenty-four hours) by clockwork, and is tilted so as to be parallel to the earth's axis. In this position the collimator points to the north pole, and the camera to the south pole.

The tube of the collimator is prolonged beyond the plane of the slit, and is arranged to carry at its end a mirror of speculum metal and an object glass, by means of which an image of the sun can be thrown upon the slit

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* 'Roy. Soc. Proc.,' vol. 64, p. 56.

The whole arrangement thus consists of a spectroscope combined with a polar heliostat and in virtue of the fact that the spectroscope is rotated together with the mirror, the image of any celestial object thrown upon the slit does not rotate relatively to the slit Furthermore, the mirror is mounted in such a manner that the axis about which it can be tilted—namely the declination axis can be oriented relatively to the collimator tube so that any diameter of the sun may be set parallel to the slit

A special plate holder was designed for use in Algiers in order to facilitate the rapid change of plates It was charged with twelve plates, fixed film outwards on the outside of a cylinder (2 inches in diameter), whose axis was set parallel to the focal plane of the camera and in the plane of dispersion free to turn inside a slightly larger covering cylindrical case The arrangement was turned by hand, and worked admirably well It is, however, only suitable for narrow spectrum plates, and might be used with very small alteration for a film on celluloid, such as is used in hand cameras of the Kodak type

The linear dispersion in the photographed spectrum is about 14 tenth metres per millimetre at H_γ The width of the slit was adjusted to 0·03 mm by a diffractional method

The scale of the photograph is such that one degree on the sky corresponds to about 9 mm on the plate

The effective aperture of the combination regarded as an instrument for producing monochromatic images of a slit-shaped region of the corona is *f*/10

The adjustment of the axis of the instrument to parallelism with the earth's axis was accomplished in the same way as in India by means of a theodolite with declination circle and level, which was attached to a part of the frame of the spectroscope specially prepared for it

Programme of exposures, &c —

I *Spectrum of the Sun's Limb at the Beginning of Totality*—Five exposures were made in 7 seconds, beginning 3 seconds before Professor Turner's signal "Start" was called, and ending as Mr Wyles called the "fifth" beat of the metronome

Result—The developed photographs show that the first plate was exposed at exactly the right moment to catch the spectrum of the "flash" It is filled with bright lines, and shows the part of the spectrum between H_ζ (3900) and H_β (4861) The best part of the spectrum is that between wave lengths 4100 and 4650

All the other four plates show bright lines, but the fall in the number of them is very abrupt between the first and the second plates

II. *Spectrum of the Corona*—Six seconds after Professor Turner's signal "Start" a plate was exposed for the spectrum of the corona, and the exposure was continued for 49 seconds, ending when Mr. Wyles called "fifty-five."

Result.—The developed photograph shows the spectrum of the corona in two regions situated at the ends of a chord whose length is approximately equal to the radius of the moon's image.

The radial extension is even less than in Captain Hills's photograph taken at Pulgaon in 1898. There is an abrupt fall in the intensity of the marked continuous spectrum at about 2½′ from the limb, and at 3¼′ from the limb the spectrum is invisible.

In the preliminary examination of the spectrum none of the ordinary Fraunhofer lines have been detected, a fact which is of remarkable import when considered in connection with the intensity of the polarisation of the light emitted from the corona. (See below, p. 364.)

In one of the spectra the hydrogen lines are very strong, viz., H_β, H_γ, H_δ, H_ϵ, and H_ζ. In the other they are barely visible, H_γ appearing only very close to the limb, and not extending more than about a quarter of a minute of arc. The helium lines are strong in one and barely perceptible in the other. In one the calcium lines H and K are intensely strong and broadened, though the edges are defined and the lines very much shifted towards the red end of the spectrum; in the other, the H and K lines are weak and well-defined narrow lines. It is important to note that the shift of the broad calcium lines is in the direction that one would anticipate if pressure were the cause of the broadening and of the shift. Whilst it seems clear that the presence of the hydrogen, helium, and calcium lines in one and not in the other of the two regions of the corona whose spectra have been photographed is probably due to a prominence, this explanation is difficult to reconcile with the signs of pressure above referred to.

There are several bright coronal lines discernible in both spectra; and in the neighbourhood of one of the lines, viz., that of wavelength 4231, there seem to be two dark lines, apparently the only absorption lines visible in the spectrum.

III. *Spectrum of the Sun's Limb at the End of Totality*.—Immediately after the end of the exposure of the plate for the spectrum of the corona, the image of the corona was readjusted on the slit, under unexpected difficulties however on account of the faintness of the light. Mr. Wyles called "sixty-four" as I reached the platform again to make the exposures, and the exposures were made as follows :—

Plate No. 7 at 65
„ 8 „ 66
„ 9 „ 67
„ 10 „ 68
„ 11 „ 69
„ 12 „ 70

and I gave the signal to Mr. Henn for his last exposure at 71.

It was found later that the faintness of the light was caused by a dark glass in front of the eye-piece, which was used for viewing the image on the slit. This was needed in the first exposures, but should have been removed by turning the hinged glass aside. It is evident from the photographs that the image was improperly adjusted in consequence of the faintness of the light; there is no impression on the plates.

The results obtained with the four-prism spectroscope may be summarised as follows · Five photographs of the spectrum of the vapours near the sun's limb at a fixed point, and a photograph of the spectrum of the corona at two points widely separated near the sun's limb.

§ 25. *The Photographic Camera with Large Objective Grating.*

Visual observations of the green coronal ring made at Pulgaon, India, 1898, January 22,* convinced me that the ring could have been photographed with the objective grating and telescope then used. Accordingly preparation was made to attempt a photograph with a large grating at Algiers. For this purpose, use was made of a plane grating by Rowland, 14,438 lines to the inch on a ruled surface 5 × 3½ inches, fitted on an axis in front of a telescope of focal length 68 inches and aperture 4 inches. The grating is a very brilliant one, and is ruled on an unusually fine-grained piece of speculum metal. The object glass is an excellent one by Cooke and Sons. Both of these belong to the splendid spectroscopic installation arranged by the late Professor Piazzi Smyth, with the aid of contributions from the Government Grant. The installation is now set up at the Cambridge Observatory, having been put at my disposal for spectroscopic investigations by the Royal Society. I am thereby put under a great obligation to the Society, and I venture to take this opportunity of making acknowledgment of it.

In the recent eclipse the sun was about as far to the north of the celestial equator as it was to the south in the Indian eclipse of 1898; accordingly the grating and telescope could be mounted in almost the same relative positions in Algiers as in India; it was only necessary to reverse the positions along the polar axis, and arrange that the telescope pointed towards the south pole instead of the north. Accordingly the instruments were mounted so that the telescope was parallel to the earth's axis and pointed downwards towards the south pole. For the purposes of taking photographs this position was extremely convenient.

A strong wooden bridge or yoke was fitted to the object glass end of the tube of the telescope, and projected in front of the object glass

* 'Roy. Soc. Proc.,' vol. 64, p. 58; and 'Mon. Not., R.A.S.,' vol. 58, App., p. (58).

at such a distance from it that the grating could be mounted free to turn on a spindle passing through the sides of the yoke at right angles to the collimation axis. A small brass cup or socket was attached to the middle point of the yoke so that it lay in the axis of collimation, and it was made the lower bearing, by which the whole instrument was supported on a pointed pivot, fixed, with a small amount of freedom for adjustment, on a low pillar of brickwork. The upper end of the tube of the telescope rested on antifriction rollers, supported on the west side of the higher pillar of brickwork, which also carried the four-prism spectroscope. Thus the polar axes of the two instruments, viz., the objective-grating camera and the mounting of the four-prism spectroscope, were side by side, and it was not a difficult matter to link together the two mountings by means of a connecting rod, so that the same clockwork should drive both. Each mounting was connected by slow motions with the one clock-driven sector, and so each could be adjusted relatively to the sun without disturbing the other. The arrangement worked admirably.

The light of the corona was incident on the grating at an angle of about 55°, and the diffracted beam utilised in the telescope left the grating at an angle of about 13° 40′. In this position of the grating the green of the second order was used and the magnifying power of the grating was a little greater than one-half, so that the coronal ring was distorted into an ellipse, in which the major axis was perpendicular to the length of the spectrum and parallel to the direction of daily motion.

The axis of the instrument having been adjusted to parallelism with the earth's axis, it remained only (i) to set the grating so that the coronal ring should appear in the middle of the field, and (ii) to focus the instrument. Neither of these operations could be done satisfactorily before the eclipse, that is, before the diminishing crescent of the sun made it possible to recognise the exact position of the spectrum in the field of view. Ten minutes before totality the dark lines were indistinctly visible in the spectrum, and a glance showed me that I had had an extraordinary stroke of good fortune in the rough setting of the grating, an operation which had been done by turning the grating till I thought the colour of the green was about right for the background of the magnesium lines. For the lines were only slightly displaced from the centre of the field, and the adjustment for the part of the spectrum required in the photograph was practically correct to a nicety. Accordingly no further adjustment was attempted. Two minutes before the beginning of totality the crescent was fine enough to show the dark lines in the spectrum very distinctly, a somewhat bewildering array of interlacing elliptical crescents, and the focussing was accomplished with ease. Mr. Henn then took charge of the instrument, and put a dark slide in position, and adjusted the exposing shutter. I am very much indebted to him for his admirable precision in carrying out the programme of exposures.

The programme was carried out as follows :—

Three plates were exposed.

Plate X, 1. For the brightest chromospheric lines, at the beginning of totality—a short exposure, about 1½ seconds.

This plate was to be exposed at the signal "Start," given by Professor Turner, and was to be closed between the time-keeper's calls "one" and "two."

It was actually exposed at the signal "Start," and closed at the time-keeper's call "three." The time-keeper found that the first beat of the metronome after "Start" came so soon that he did not call "one," but called the next beat "two" without calling "one" at all. The exposure was thus probably 2½ seconds.

Plate X, 2. For the green coronal ring—a long exposure, about 40 seconds.

This plate was to be exposed as soon after Plate X, 1 as the change of plate holders would allow, and was to be closed at the call "fifty-five."

It was actually exposed at "nine" and closed at "fifty-five," and thus had an exposure of about 46 beats.

Plate X, 3. For the Fraunhofer lines immediately after the end of totality for comparison with any chromospheric lines that might appear on Plate X, 1.

This plate was to be exposed when I gave the signal "Now," and was to be closed 1 second later.

It was actually exposed at "seventy-one," and closed at "seventy-two."

Results.—The Plates X, 1 and X, 2 show faint images, but have not been examined carefully yet; a cursory examination shows that (*a*) only a single chromospheric line appears on X, 1; and (*b*) continuous spectrum appears on X, 2, but no *marked* coronal ring is discernible.

Plate X, 3 is a strong spectrum, showing the curved Fraunhofer lines between wave-lengths 5050 and 5460; the linear dispersion on the plate is, roughly speaking, 5 tenth-metres per millimetre.

Remarks.—In an eclipse with longer duration of totality, the procedure here described should give good results for the green coronal ring. The plates used were Edwards's Isochromatic Snapshot plates. It should be remembered that the effective aperture of the camera, viz., a little less than F/17, was rather dangerously small.

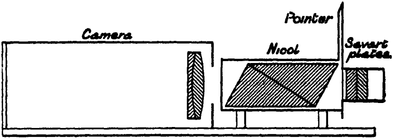
§ 26. *The Polariscopic Camera (Savart Plates and Nicol Prism).*

The glimpses of the corona that I was fortunate enough to get in India in 1898 through a small Savart polariscope convinced me that that instrument, if properly used, would give just the information that is wanted to decide some of the perplexing points that still survive in the spectroscopic and polariscopic study of the corona. The chief objection is that the phenomena are far too complicated to study by eye observations in the short time at one's disposal in an eclipse. Here is a case in which photographic methods should certainly be adopted if possible. Shortly after my return from India in 1898 I made some experiments to test the feasibility of photographing Savart's bands, and met with such promising success that no doubt was left in my mind that a photographic record of the distribution of Savart bands over the corona would give good results in supplementing the work which Professor Turner has in recent eclipses been carrying out in studying the polariscopic phenomena of the corona. Accordingly when I was asked to take part in the observations of the eclipse at Algiers, it seemed well to make arrangements to prepare a polariscopic camera.

Professor J. J. Thomson very kindly put two large Nicol prisms at my disposal. The aperture of these prisms is 1⅝ inches. It was thus possible to use a lens of a focal length of about 3 feet, if suitable Savart plates could be found. On making the necessary calculations I found that the plates would have to be 15 mm. thick, cut in quartz at 45° to the axis of the crystal. A pair of such plates would give bands of the desired closeness, viz., about 10′ apart, instead of the usual 1° or 1° 30′. Fortunately Mr. Hilger was able to cut a slab, from the sloping top of a quartz crystal that I had in my possession, large enough to make two plates, each 14 mm. thick, of circular section, and with a diameter of 39 mm. (1½ inches). The whole slab was worked and polished with plane parallel surfaces, so as to secure equality of thickness, and was then cut into two parts, which were combined in the usual manner.

The figure shows diagrammatically the arrangement of the camera with the Savart plates and Nicol prism in front. The lens was a 3½-inch lens of focal length 40 inches. The aperture was reduced to 1½ inches, or approximately F/27 for central pencils. The Savart plates were fixed to the Nicol prism so that the bands were parallel to the plane of polarisation of the light transmitted by the Nicol. The whole system was arranged so that it could be rotated on its axis into any desired position, and a pointer was provided so that the position could be read off a large circle.

In discussing with Professor Turner the arrangements for the various items in the programme of observations to be carried out, he



very kindly suggested that the parts of this apparatus should be put into one of the compartments of the "double tube" alongside of the other polariscopic apparatus which he had himself arranged I fell in with this suggestion very gladly, and the parts were taken to Algiers to be fitted there It required very careful arrangement to get the two lots of apparatus into the tube, but in the end it was successfully accomplished, and Professor Turner made the exposures for the Savart camera simultaneously with those for his own polariscopic and other cameras The pictures obtained with the Savart camera are on the same plates with the pictures obtained with Professor Turner's double image polariscopic camera

The general procedure with the Savart camera was to be as follows —The Savart and Nicol were to be rotated until the bands due to the plane polarisation of the sky in front of the corona were extinguished, and photographs of the corona were to be taken But it was not possible to look through the camera itself in order to make the adjustment "to extinction," for this would have interrupted the exposures for all the other instruments in Professor Turner's charge Accordingly a subsidiary Savart polariscope was provided, which I may call the visual Savart to distinguish it from the camera Savart The visual Savart was set up in my hut, with pointer and graduated circle attached, and the zero and numbering of the scale were adjusted so that the readings corresponded with those of the camera Savart, account being taken of the fact that the sky was seen in the camera by reflection from the cœlostat

The programme of exposures was as follows —

1 second, 5 seconds, 20 seconds, 5 seconds, 1 second

The first two were made with an arbitrary setting of the Savart, and the setting chosen was approximately that which would correspond to extinction of bands due to vertical polarisation Meanwhile I had determined the plane of polarisation of the sky in front of the corona by observations with the visual Savart, made immediately after the exposures with the four prism spectroscope were so far completed that the long exposure for the corona spectrum was begun, viz , 6 seconds after the signal "Start." Mr. Potter, standing by me, received the reading resulting from my observations, and carried it to Professor Turner's hut, and Major Kingsley Foster adjusted the large Savart to the corresponding reading, and the third exposure was begun.

The camera Savart was left with the pointer at 10° for the rest of totality, no attempt being made to test the permanence in the position of the plane of polarisation of the sky as the total phase of the eclipse passed over.

Results.—The resulting photographs show strong bands over the corona. A cursory examination discloses the following results :—

No. 1 1 second. Coronal extensions discernible as far as 10′ or 11′ from the limb

No atmospheric bands visible, but obvious bands over the corona.

No. 2. 5 seconds. Coronal extensions as far as 35′ from the limb.

The planet Mercury appears on the plate

Atmospheric bands are visible, very faint, on the following side of the sun, extending 4° 40′ from the limb, but are not visible on the preceding side near Mercury.

No. 3 20 seconds. Coronal extensions 63′ in N*p* streamer.
" " " " 52′ in S*p* streamer.
" " " " 70′ in N*f* streamer.

Mercury very strong.

Atmospheric bands visible to the edge of the plate on both sides. Strong bands over the corona.

No. 4. 5 seconds. Coronal extensions 35′ from the limb.

No atmospheric bands visible on either side.

Nos. 5 and 6. Not examined.

The existence of the image of Mercury on the plates will be of great value in determining orientation in the polariscopic phenomena as well as in the corona.

The strong bands over the corona indicate that a considerable portion of the light is polarised. There are irregularities in the bands which seem likely to afford interesting study just in the way that was anticipated.

The atmospheric bands faintly visible on the plates are almost certainly due to imperfect adjustment of the Savart to extinction, arising from zero errors, &c.; they might be due to a change in the position of the plane of polarisation of the sky after the initial setting of the Savart. In any case they are very feeble, and it is clear that it would be well, if ever the experiments are repeated, to *aim* at imperfect adjustment, so that the atmospheric bands may be in opposite phase—*i.e.*, with black central band—to the coronal bands.

By a very fortunate accident just such an imperfection has arisen in the case of the plate No. 3, for the bright bands on the corona fall on dark atmospheric bands. It might be that the curvature of Savart's bands, which theoretically exists, misleads one; but a tolerably careful examination of the faint bands shows them to be sensibly straight in the limited field dealt with, and the antagonism of the bands leaves no possible doubt that the bands seen on the corona are due to the polarisation of the corona.

It is difficult to reconcile the marked polarisation evidenced in this investigation with the absence of Fraunhofer lines in the spectrum of the corona.

Across the dark moon no atmospheric bands are discernible, and there appears to be no doubt that photographically the dark moon is darker than the sky. These are points that need explanation. An investigation of the real facts would be difficult, but none the less interesting; for the idea suggested by much of the evidence along different lines is that some of the light which is usually attributed to the sky may come from beyond the moon. For instance, is a milky sky on a moonless night simply the result of starlight scattered by the processes producing scintillation, or are other causes at work?

§ 27. *Atmospheric Polarisation.*

Preparations had been made that a systematic survey of the polarisation of the sky should be undertaken during the eclipse, with a view to determining the plane of polarisation in various quarters of the sky, a more precise knowledge of the general distribution of polarisation being needed for the explanation of some of the anomalies that appear to have been observed with respect to the atmospheric effects in previous eclipses.

Nine Savart polariscopes were mounted in similar turning tubes, provided with pointers and graduated circles, and attached to wooden stands. The stands were arranged so that each carried two polariscopes; one pointed to the horizon, the other to a point 30° above the horizon. The four stands were fixed on the top of a tall box on the balcony of the equatorial coudé which M. Trépied had kindly put at our disposal. The polariscopes were directed towards the four quarters of the sky, N.E., N.W., S.W., and S.E. During the eclipse the sun was at an altitude of 30°, and only a few degrees north of west; thus the polariscopes were directed to points nearly symmetrically disposed with regard to the sun. All the Savart plates were fixed relatively to the Nicol prisms, so that the bands were parallel to the plane of polarisation of the light transmitted by the Nicols.

The ninth Savart polariscope was mounted in a turning tube with pointer and circle complete, on a board which was screwed to an inclined block on the western doorpost of the hut which contained my spectroscopic apparatus. It was pointed towards the corona, and was in fact the visual Savart used by myself in the way described in the previous section (p. 363) for *determining the position of the plane of* polarisation of the light from the sky in the immediate neighbourhood of the corona, so that the camera Savart could be adjusted accordingly. The polariscope had been left in position with the bands horizontal and the pointer at 90°. Six seconds after the beginning of totality I left the spectroscope and looked through the polariscope. The eclipsed sun was slightly (perhaps 5°) to the north of the centre of the field of the Savart. The bands were seen fairly strong over the whole field of view, the central band being black. The Savart was then turned counter-clockwise, until the bands were extinguished. The reading was found to be 9° on the scale arranged to correspond with that on the large Savart in Professor Turner's hut. This reading showed that the plane of polarisation of the sky in front of the corona was inclined at an angle 4° to the vertical read counter-clockwise from the vertex. (There is possibly a zero error; it has not yet been determined.) When the atmospheric bands were extinguished faint traces of bands were seen over the corona, but much less strong than in the Indian eclipse.

I examined the polarisation of the sky in the zenith about 10 seconds after the end of totality, and found that the plane of polarisation passed through the sun.

Mrs. Newall undertook the charge of the eight other polariscopes, which were arranged as described above, and her programme for the eclipse was to turn each instrument, so that the Savart bands disappeared, paying attention to the direction of turning as follows:—If the band system had a white central band the polariscope was to be turned clockwise. If the system had a black central band it was to be turned counter-clockwise. The instruments were then to be left untouched, and the positions of the pointers were to be written down at leisure after the eclipse. Mrs. Newall devoted herself very diligently to setting the instruments under very varied conditions on the days preceding the eclipse, and so expert did she become that she was able to make the necessary settings of all eight polariscopes in about 42 seconds. If the polarisation was weak, about 50 seconds were needed. In the following table, taken at random from her notebook, the figures in the upper line are the readings of the pointers of the various polariscopes when the settings were made in a leisurely manner; those in the lower line are the readings when the settings were made "racing." "Hor." refers to the horizontal polariscope; "30°" to that which points upwards :—

S.E.		N.E.		N.W.		S.W.		
Hor.	30°	Hor.	30°	Hor.	30°	Hor.	30°	
106°	74	168°	178	170°	121	114°	136	Leisurely.
104	74	164	176	164	122	116	135	Racing.

The following readings show that in a leisurely setting the observation of extinction of the bands is satisfactory.

1900. May 24. Ten settings for extinction of bands in the middle of the field—16°, 15°, 16°, 12°, 16°, 17°, 16°, 17°, 17°, 16°. Mean 15°·8. It is thus clear that the "racing" settings give results of about the same order of accuracy as the leisurely ones

In the eclipse itself, the observations were made on the balcony of the equatorial coudé, and unfortunately, on account of other noises, the signal, "Stand by," announcing that the beginning of totality was approaching, was not heard by Mrs. Newall, who was standing in the doorway of the balcony with a view of protecting her eyes from the sunlight till the last moment. Nearly half of the duration of totality had passed before she came into the open, and heard the twenty-eighth beat of the metronome being called. Going at once to the polariscopes she began to adjust them; she had set four of them "to extinction," and had nearly completed the setting of the fifth, when sunlight reappeared. With regard to the last observation, Mrs. Newall noted an interesting point. She had nearly completed the setting to extinction when the bands suddenly became bright again, with black centre, and she turned the polariscope counter-clockwise, from somewhere near the reading 20°, and had nearly set again to extinction before realising that totality was over. The reading of the polariscope was then found to be 345°.

The actual circle readings recorded immediately after the eclipse were as follows, and it was noted that the bands were very faint:—

S.E.		N.E.		N W.		S.W.
Hor.	30°.	Hor.	30°.	Hor	30°	
105°·0	94°·1	324°·8	340°	?20° ±		

[345° after return of sunlight.]

These require small corrections for the index errors, which can only be determined after the instruments return from Algiers, but it may be provisionally stated that the angles made by the plane of polarisation with the vertical, read from the vertex clockwise, are as follows:—

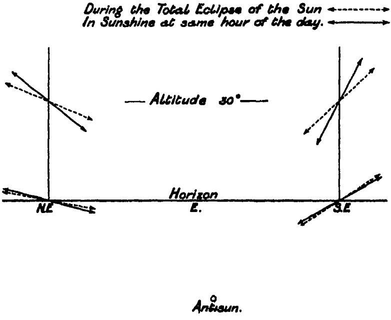
S.E.		N.E.		N.W.		W.
Hor.	30°.	Hor.	30°.	Hor.	30°.	Over corona.
60°	49°	280°	295°	?305° ±		356°

[From my own observations.]

Comparison with the observations secured on other days at about the same time of day as the eclipse, viz., 4.30, may be summarised graphically as in the accompanying figure.—

Polarisation of the Sky Algiers, 1900, May 26–28

Plane of Polarisation.


During the Total Eclipse of the Sun
In Sunshine at same hour of the day.
— Altitude 30° —
Horizon
NE
E.
SE
Antisun.

The results are of considerable interest in their bearing on the well-known peculiarities in the phenomena of the polarisation of the sky in the neighbourhood of "neutral points."

It is a great satisfaction to be able to record these observations, for though they are incomplete, yet they were successfully carried out in spite of circumstances which would have upset many a practised observer; and the regret is all the greater that Mrs. Newall had to pay such a forfeit for her resolution, for she did not get more than a glimpse of the eclipse.

Of the nine Savart polariscopes used in these observations, four were lent to me by the Council of the Royal Astronomical Society, and three by Professor Lewis, through Mr. A. Hutchinson, of Pembroke College, Cambridge, who had arranged to come to Algiers, and had volunteered assistance in the observations recorded in Section 25 of this Report, but was unfortunately prevented at the last moment, by illness, from coming.

§ 28. *General Observations.*

The general darkness during totality was about the same as in India.

The dark moon did not appear so strikingly coal black in Algiers as it did in India. This is curious when considered in connection with the fact that the polarisation of the sky in front of the corona was much stronger in India than in Algiers.

Round the limb the brightness appeared relatively much greater in Algiers than in India. The breadth of the bright ring was estimated as 2′ to 3′; the decrease in brightness along the radius was very abrupt at this distance from the limb; at 4′ or 5′ from the limb a lower level of brightness was reached and thence outwards along the equatorial streamers the decrease in brightness was small up to points about 2° from the centre.

My impression of the streamers, recalled from a very vivid memory of the picture in my mind, is that the double streamer on the preceding side of the sun certainly extended beyond Mercury, and there was a similar extension on the following side. The latter extended for some distance as a broad streamer with nearly parallel edges.

I used a telescope of 3½ inches aperture and of 29 inches focal length for viewing the corona direct. The instrument was merely clamped to the walls of the hut. I was able to focus the instrument carefully, and devoted some moments to examining the corona immediately outside the large prominence in the *Sp* quadrant. The prominence appeared double, one side having the form of a tapering column projecting radially from the limb, and the other appearing in cloud-like floating forms, both parts being of a wonderful rose colour. In the corona I was disappointed to find no striking signs of arches over the prominence. The only fine structure visible in the corona were a few interlacing wisps crossing one another, presumably in the part where the two streamers on the preceding side of the sun crossed one another in diverging from one another.

The shout that is stated to have risen from the Arabs in Algiers was heard by me as I exposed the plate for the spectrum of the corona, that is, as Mr. Wyles called "six." Algiers lies just a little more than a mile in a direct line from the Observatory. It seems probable that the shout was uttered at the same instant as Professor Turner's signal "Start," and announced that the totality had begun at Algiers.

"Solar Eclipse of May 28, 1900. Preliminary Report of the Expedition to the South Limit of Totality to obtain Photographs of the Flash Spectrum in High Solar Latitudes." By J. EVERSHED. Read at Joint Meeting of the Royal and Royal Astronomical Societies, June 28, 1900. MS. received July 16, 1900.

This expedition was one of those organised by the Joint Permanent Eclipse Committee of the Royal Society and the Royal Astronomical Society, funds being provided from a grant made by the Government Grant Committee.

The following were the principal objects which I had in view in arranging the expedition:—

To obtain a long series of photographs of the chromosphere and flash spectrum, including regions of the sun's surface in mid-latitudes, and near one of the poles.

The photographs to be obtained with a long focus prismatic camera on a large scale, in order to be able to discriminate clearly between high levels and low levels in the chromosphere.

The photographs to include as much as possible of the ultra-violet region of the spectrum, for the purpose of verifying the results obtained with a smaller instrument in 1898, and to give more accurate values of the wave-lengths determined from those results.

This report may be conveniently divided into the following four sections, viz.:—

1. Selection of observing station.
2. Instruments, methods of mounting, and general arrangement of camp.
3. Narrative of expedition, and observations made on the day of the eclipse.
4. Results.

(1) *Selection of Observing Station.*

A consideration of the conditions under which the lowest layers of the chromosphere are presented during a total solar eclipse showed that a very great advantage would be gained by selecting a station situated near the limit of the zone of total eclipse, where the two internal contacts would be separated by a small angle on the sun's limb.

At such a station the motion of the moon relative to the sun is in a direction approximating to parallelism with a tangent to the sun's limb at the points of internal contact, the result being that the excessively shallow layer giving rise to the so-called "flash spectrum" is occulted by the moon comparatively slowly. Much more time is therefore available for taking a series of photographs than is the case at stations near the central line of the eclipse, where the moon's motion is at right angles to the layer, and opportunities for obtaining photographs of the very lowest strata are reduced to a fraction of a second only at each internal contact.

I decided therefore to choose some point situated well within the zone of total eclipse, but so far from the central line that the two internal contacts would be separated by an angle of about 39° on the sun's limb. This would give a duration of totality equal to one-third that at the nearest point on the central line; and the time available for photographing the flash spectrum would not be less than 30 seconds. At mid-eclipse the moon's limb would overlap the photosphere about 1″, so that even at that time the flash spectrum layer would not be entirely hidden.

Under these circumstances, also, one of the contacts would take place at, or very near to, one of the poles of the sun, the other being in latitude 51°. A succession of photographs taken during totality would therefore give a series of images of the flash spectrum ranging from solar latitude 51° to the pole.

In selecting the most suitable station, dryness of climate was considered to be the most important factor for securing extension of the spectra in the ultra violet; I therefore selected Algeria in preference to Spain, although the altitude of the sun would be less in the former country.

The best position in Algeria for realising the greatest solar altitude was a point on the coast west of Algiers, and on the southern border of the eclipse track. This region was therefore decided upon at the outset, and in order to realise the favourable conditions mentioned above, two stations were selected provisionally beforehand, and for these Dr. Downing kindly computed for me the durations of totality according to the data used by the Nautical Almanac Office.

The first station, near to the village of Zeralda, was found by him to have a duration of 45 seconds, *i.e.*, more than one-half the central line duration. The other station, three miles further south, and near to Maelma, was computed to have a duration of 29·5 seconds with a possible error of ± 10 seconds.

As the required conditions would, apparently, be very nearly fulfilled at the latter station, I decided to place my camp either at that precise spot, or at some point situated on a line passing through it, and parallel to the direction of the shadow track in that region.

The actual station eventually chosen was 6·5 kilometres distant from the station near Maelma, in a direction bearing West 25° North. Here it was estimated that totality would last 30 seconds. Unfortunately, as the event proved, the value of the diameter of the moon adopted by our Nautical Almanac Office is too large, and the limits of error given above were very misleading. Instead of a duration of 30 seconds, the eclipse at my station was never quite total.

(2) *Instruments, Methods of Mounting, and General Arrangement of Camp.*

It was my intention originally to take out a fine 18-inch silver-on-glass concave mirror made by the brothers Henri, which was given to me by the late Mr. Ranyard This mirror, having a focal length of 117 inches, would have given images on a scale of 1·08 inch to the sun's diameter

Many experiments were made with this mirror to determine the amount of aberration produced on star images at considerable distances from the axis, and with various apertures. It was found that when the ratio of aperture to focal length did not exceed 1/15, good images were obtained 4° from the normal axis, the aberration being very slight.

As this would admit of a very wide range of spectrum being photographed with good definition throughout, I decided to adapt my large reflecting telescope for eclipse work. Owing, however, to the difficulty of obtaining a prism of large angle and not less than 6 inches aperture, I had, most unfortunately, to abandon this scheme and construct a much smaller apparatus.

Through the kindness of Dr. Rambaut I eventually obtained a fine 4-inch prism of light flint glass and 45 degrees angle. This prism, which was generously placed at my disposal by Sir Howard Grubb, proved most efficient for the work, although I was unable to utilise the full aperture.

Three spectrographs were finally made: a reflecting prismatic camera of 3 inches aperture and 74 inches focus, an ordinary prismatic camera of 2 inches aperture and 47 inches focus, and a quartz prismatic camera of 1 inch aperture and 24 inches focus. These were mounted together inside an observing hut, and were supplied with light from a 12-inch cœlostat.

The Reflecting Prismatic Camera.

This was an ordinary reflecting telescope with a mirror of 9 inches aperture and 74 inches focus. It was fitted with a strong wooden tube, adapted for carrying two large prisms near the upper end. The prisms used were the 4-inch 45-degree prism, lent me by Sir Howard Grubb, and a 3-inch 60-degree prism lent me by Dr. Common. These were mounted eccentrically within the tube, in such a manner that the incident light, after passing through the prisms, made an angle of about 1½° with the normal axis of the mirror. After reflection from the mirror the rays returned over the upper surface of the 60-degree prism, and came to focus about an inch outside the end of the tube.

The end of the tube was closed by a block of wood having an aperture 8 inches long by 3 inches wide, a little above the middle Outside this a long slide was arranged at right angles to the telescope, and bolted at the upper end to two stay rods attached to the telescope near the mirror

A plate holder 3 feet long by 10 inches wide taking two plates 8½ inches square and four plates 8½ by 4¼ inches was arranged to move along the slide by means of rackwork and a pinion wheel One revolution of the pinion moved the plates 2 13 inches whereby four images could be obtained on the square plates and two on each of the narrow ones the sixteen images all being equal distances apart and symmetrically placed on the plates The revolutions of the pinion wheel were controlled by a spring catch acting on the crank handle, and holding it firmly in position after each revolution

The whole slide carrying the plate holder, &c was attached to the telescope in such a way that the distance of the plates from the mirror could be varied a small amount for focussing

The tube of the instrument was firmly bolted down to the sloping side of a solid pier of stone and cement built up within the observing hut near the north end It was adjusted so that the plane of dispersion of the prisms was in a meridian passing through the cœlostat, and inclined to the prime meridian 68 (the hour angle of the sun at mid eclipse) The dispersion was therefore in a north and south direction The internal contacts were computed to occur near to the south point of the sun, and on either side of it The centre of the flash spectrum arcs was therefore midway between the edges of the spectrum in the photographs obtained at mid eclipse

The 2 inch Prismatic Camera

This instrument was the same which I employed successfully at the Indian eclipse in 1898, excepting that it was fitted with a specially corrected lens of 47 inches focus instead of the visual objective previously used The images were therefore on a somewhat larger scale, and larger plates were used

The sliding plate holder, constructed on the same lines as the larger instrument already described, was made to hold three plates, 6½ by 4¼ inches, placed lengthwise in the holder, and the crank handle moving the slide was arranged to stop at each half revolution, moving the plates 1 12 inches between each exposure

The two 60 prisms of this instrument are made of specially selected crown glass, and are exceptionally transparent for ultra violet rays

The total deviation of the two prisms being approximately equal to that of the reflecting spectrograph (about 80) the tubes of the two instruments were arranged nearly parallel, the 2-inch spectrograph being screwed to the side of the reflector with its aperture alongside that of the latter. The camera end with the sliding plate holder was at the lower end.

The Quartz Prismatic Camera.

This was rigged up while in camp, as it was found that a small portion of the cœlostat mirror was available to supply light. It consists of two double quartz prisms of 60 and 40° angle respectively, each prism having 1¼-inch square faces, and a single quartz lens of 24 inches focus It was screwed on the top of the 2-inch spectrograph, with its aperture just within the elliptical beam of light from the cœlostat.

My brother arranged a very convenient exposing shutter, which he was to open near mid-eclipse for a single exposure of 10 seconds.

Methods of Focussing.—All three spectrographs were approximately focussed by taking a series of photographs of the spectrum of Venus. Having determined the focus of the reflecting prismatic camera in this way within very narrow limits, I used this instrument as a collimator for the 2-inch spectrograph, removing the large prisms, and adjusting a slit in the position occupied by the plates when photographing Venus. The 2-inch instrument was attached to the wall of the hut, with its aperture inside the tube of the reflector, and directed towards the mirror. With the north door of the hut widely open, the slit was illumined by light from the sky, and a series of photographs was obtained of the Fraunhofer spectrum.

For some reason the focus was not so sharply defined, and the definition of the lines was not so good as in photographs I had obtained by the same method before leaving England.

A third method was therefore tried. After having adjusted the 2-inch spectrograph in its correct place, photographs were obtained of the sun itself on Sandell triple plates; the focus being determined by the images which were most sharply defined along the edges.

A final method, which was adopted for the two larger instruments, was to adjust the focus visually during the eclipse itself, using the Fraunhofer lines, which become sharply defined shortly before totality.

Programme of Exposures.—The two larger prismatic cameras were each to have sixteen exposures made simultaneously, by removing a plate of aluminium from the common aperture of the two instruments. The first, second, fifteenth, and sixteenth exposure were to be of about 1 second duration each, and the remaining exposures of 2 seconds duration, excepting the exposure nearest to mid-eclipse, which was to have 10 seconds.

The quartz prismatic camera was to be exposed near mid-eclipse also for 10 seconds.

These relatively long exposures were designed to secure density in the ultra violet, at the risk of over exposing in the region near G.

Having ascertained by rehearsing that the time required for exposing all the plates would be about 90 seconds I arranged that the first exposure should be timed at 15 seconds before the computed time of mid eclipse. The succeeding exposures were to follow each other at the shortest intervals turning the handles deliberately and allowing ample time for shake to subside before each exposure.

I was to use my discretion to some extent in making the long exposure at mid eclipse but otherwise I intended to be guided solely by the chronometer.

The exposures were to be made by myself standing at the north door of the hut and facing the large spectrograph. I used my left hand to work the exposing shutter and my right to rack the plates forward in the slide.

My brother sitting on his bed in the hut was to move the plates of the 2 inch spectrograph turning the handle half a revolution after each exposure. He was also to expose the quartz spectrograph at a signal from me.

The Cœlostat

A 12 inch cœlostat was used to supply light to the three spectrographs in the hut. It was placed about 6 feet from the north east corner of the hut and was arranged to reflect the sun in a meridional plane, the angle between the incident and reflected beam being in this case a minimum, viz declination of sun × 2. The reflected beam was in a direction W 30° S, and was directed upward at an angle of 3½ with the horizontal.

The instrument was mounted on a steel plate fixed on the top of a masonry pier, and about 3 feet from the ground. The plate had a straight channel cut in it just wide enough to take the ends of two of the four levelling screws, the other two resting on the planed surface of the plate.

With the plate placed approximately level and the channel approximately north and south, the whole instrument could be shifted bodily north or south without disturbing the adjustments of the axis in altitude and azimuth. In this way the adjustment of the beam of light with respect to the apertures of the three spectrographs was very easily managed, and the cœlostat could be shifted about to suit the varying declination of the sun on occasions, previous to the eclipse, when it was desired to observe the spectrum and adjust the spectrographs.

The cœlostat was provided with slow motion independent of the driving gear, and this was controlled from inside the hut by means of a rod, 8 feet long, which my brother laboriously cut from a 3-inch plank

The driving clock was bolted down to the north end of the cœlostat pier. It was driven by a weight suspended from a large tripod erected near.

The 3½-inch Tel

In addition to the photographic instruments I had a 3½-inch equatorial telescope, and a high dispersion solar spectroscope. The telescope was mounted on a packing case outside the hut, and was useful in a variety of ways. With the spectroscope attached it was used for the observation of solar prominences on the day of the eclipse, and on several other occasions.

The Observing Hut.

This was a rectangular wooden shed, the sides enclosing a space 14 by 9 feet and 6 feet high. The sloping roof was covered with boards up to about 1 foot from the centre beam. A large sheet of canvas was stretched above the boards, leaving an air space between: this allowed of the free circulation of air between the canvas and the wood, and was designed to prevent the interior from becoming unbearably hot during the day.

This arrangement, however, was anything but water-tight, as we found to our cost during a spell of bad weather. We subsequently procured a large rick cover, which was tied securely over the canvas and down to the ground on the weather side of the hut.

This hut was designed by my brother for the accommodation of the somewhat unwieldy reflecting spectrograph and two camp beds. The frame was constructed by him before leaving England.

The accompanying ground plan shows the general arrangement of the camp.

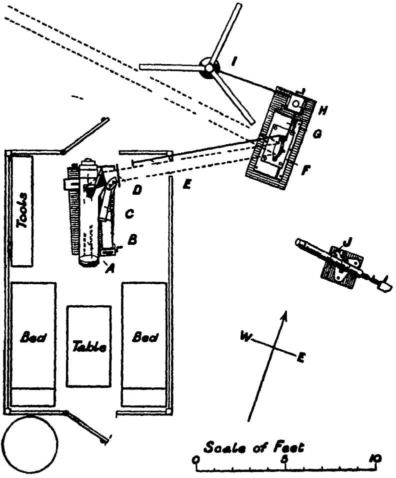
(3) *Narrative of Expedition and Observations made on the Day of the Eclipse.*

The expedition, consisting of my brother (Mr. Harry Evershed) and myself, left England on April 30th, and travelling *via* Paris and Marseilles arrived at Algiers on May 3rd.

At Algiers we received every attention and assistance from the British Consul-General, Mr. Hay Newton, to whom our acknowledgments are due. He procured for us a letter from the Préfecture to the Mayor of Maelma, ordering the latter to assist us in every possible way in selecting a site for our camp.

We also received assistance and advice from M. Trépied, of the Algiers Observatory, who very kindly called at our hotel and discussed with us our plan of operations.

PLAN OF OBSERVING STATION, MAZAFRAN CAMP



A Reflecting spectrograph
B 2 inch spectrograph
C Quartz spectrograph
D Exposing shutter
E Coelostat slow motion handle

F Coelostat
G Planed steel plate with channel
H Driving clock
I Tripod for weight
J 3 inch telescope and spectroscope

Having obtained letters of introduction to some landowners in the district we intended to occupy, we went at once to the village of Zeralda, about 20 miles west from Algiers, and making this our head quarters for a few days we explored the neighbouring country

The people to whom my brother had letters received us with the greatest civility, and we desire to mention in particular M Buloff, administrator of the estate of the Comte de Perigord and formerly Professor at Stonyhurst This gentleman was much interested in our mission and we are practically indebted to him for giving us a letter to a colonist M Alvado upon whose farm near the sea coast we eventually found an excellent site for our camp

It was evident at the outset that the wild hilly region near the village of Maelma would be more difficult of access than the country further west near the coast Maelma itself we found to be poverty stricken and unpromising The mayor whom we found in his mairie busy with the coming elections was obliging enough to *visé* our document from the Préfecture or rather he got his secretary to do so being unable himself apparently to read or write Having had our letter duly *visé* we abandoned Maelma and proceeded to the Mazafran River near the coast to conclude negotiations already entered into with Alvado

These presented no difficulty for M Alvado was un homme très brave and offered us his whole territory vineyards or cornfields for our camping ground We were however limited in our choice to a line bearing West and 24½ degrees North from Maelma in order to secure the same duration of totality as had been computed for that place

The position finally chosen was near to the mouth of the Mazafran River on the east side and about 1 kilometre from the sea

The position of the Mazafran bridge about 400 metres distant was ascertained from a recent survey to be

North latitude	36	41	35
East longitude	2	48	30

The position of the camp which my brother carefully determined by triangulation from the bridge was as follows —

North latitude	36	41	47
East longitude	2	48	41

It was 17 metres above sea level and 6 5 kilometres from the station near Maelma, in a direction bearing West 25 North

As the direction of the shadow track in this region was ascertained to be 24 39 North of West we concluded that the above position would be safe for a duration of 30 seconds of totality

Having settled all preliminaries we returned to Algiers to arrange for the transport of the instruments This was effected without difficulty by means of the light railway recently constructed from Algiers to the Mazafran

On May 9th we returned to Alvado's farm, and the next day the work of erection was begun

Our hut, which was to serve as sleeping and living room as well as observatory, we had ready for occupation the same evening.

During the fortnight preceding the eclipse, our time was fully occupied in erecting and adjusting the three spectrographs, cœlostat, and other instruments and in taking trial photographs for determining focus We also made daily observations of the sun with a sextant and artificial horizon for determining time and checking the rate of a chronometer which we had hired for use in our camp Being far from any telegraph station in direct communication with Algiers we were obliged to depend entirely on observation for our time on the day of the eclipse Working with instruments of very second rate quality my brother usually succeeded in determining local time within one or two seconds of error taking the mean of a day's set of observations (usually employing the method of double altitudes)

During the whole time we were in camp we were ably assisted by our host, M Alvado who took a most intelligent interest in all our operations, and was ever ready and at hand to help us in any and every difficulty with which we were confronted We take this opportunity of expressing our high appreciation of his services and esteem for his character, and that of his wife Madame Alvado The latter attended most assiduously to all our personal wants and in this way furthered most materially the objects of the expedition

Observations made on the Day of the Eclipse

Between 6 and 7 A M on May 28th I observed in the spectroscope the position angles and approximate heights of all the prominences then visible on the sun's limb The results were then written out in accordance with a previously arranged code and sent on to Zeralda to be telegraphed to Mr A C D Crommelin, at Algiers, for the use of intending observers of the coronal structure near to prominences

The following table gives the position angles and heights observed —

Position angle	Solar latitude	Approximate height Hα
64	+ 9	50
110	− 46	15
217	− 36	115/130
236	− 17	25
305	+ 52	20

The rest of the morning was devoted to final adjustments and rehearsals, cleaning all lenses and prisms, and in taking more photographs for focus in the 2 inch spectrograph Soon after noon all slides were filled ready for the eclipse, and lastly, the 9 inch mirror and the cœlostat mirror were both dusted and carefully polished with rouge to remove all trace of tarnish

Fifteen minutes before mid-eclipse the large spectrograph was slightly readjusted for focus by observing with a lens the spectrum image of the diminishing crescent This was effected without any difficulty or uncertainty

I then attempted to focus the 2 inch spectrograph in the same way using the Fraunh fer lines near G which were then rapidly becoming sharply defined As the last determination made photographically appeared to be correct I set it again to the same position

Five minutes before mid eclipse my brother wound up the cœlostat clock and three minutes later I gave the order Stand by

The light waned rapidly and I began the exposures at 4 16^m 58^s At 4 17 30^s I found it difficult to see the seconds hand of the chronometer and a few seconds later I opened for the 10 second exposure giving at the same time the signal to expose the quartz spectrograph The absence of any sound from the shutter warned us that the latter had failed to act

At 4 18^m I could again see the chronometer face clearly I continued the exposures according to the programme finishing the last at 4^h 18 18^s

A minute or two later after removing all the plate holders from their slides I observed the large prominences on the south west limb in the spectroscope attached to the 3 inch telescope They appeared of course exceedingly brilliant in the line $H\alpha$ Unfortunately I was unable to make a critical examination of the spectrum for at this time a crowd of sight seers inundated the entire camp, and further observation for the time being was impossible

Later, I observed the time of last contact with the spectroscope This took place at

5^h 21^m 34^s per clock
+58 assumed error of clock
5 22 32 G M T

At this moment the moon's limb was seen as a black line projected on the chromosphere

(4) *Results*

Notwithstanding the fact that my station was outside the zone of total eclipse,* the photographs show that there was quite half a minute available for obtaining good images of the flash spectrum. No. 9 spectrum, for instance, is one of the finest of the series, and shows about as many bright lines as the mid-eclipse photograph, yet it was exposed 15 seconds before mid-eclipse. Several other photographs taken earlier than No. 9 also show a large number of flash spectrum lines.

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* From the descriptions given us immediately after the eclipse by M Alvado and others who undertook to determine accurately the duration of totality, it appeared certain that the photosphere never wholly disappeared a small point of sunlight remaining visible at the moment of mid eclipse The edge of the moon's shadow was moreover clearly seen traversing the sea and the sand dunes a short distance north of our camp, which escaped the shadow by a few hundred metres only

I think this result demonstrates the very great advantage gained at stations near the limit of total eclipse for studying this spectrum.

In cleaning the lens of the quartz spectrograph shortly before the eclipse, I unfortunately jammed the exposing shutter in such a way that it would not work at the critical time, and no photograph was obtained with this instrument.

Sixteen photographs were obtained with the 2 inch spectrograph, and sixteen with the reflecting spectrograph. The following table gives the approximate times of exposure, and the plates used in each instrument:—

Exposure No.	Approximate times.				Plates used.	
	Beginning.			Duration.	Reflecting spectrograph	2 inch spectrograph
	h.	m.	s	sec.		
1	4	16	58	½	Sandell Triple	Sandell Perfect.
2		17	5	1	" "	" "
3			10	2	Sandell Perfect	" "
4			14	2	" "	" "
5			18	2	Edwards's Ordinary Medium	" "
6			23	2	" "	Imperial Ordinary (Backed)
7			27	2	" "	" "
8			31	2	" "	" "
9			35	2	" "	" "
10			40	2	" "	" "
11			45	10	" "	" "
12		18	0	2	" "	Sandell Triple.
13			4	2	Sandell Perfect	" "
14			9	2	" "	" "
15			13	½	Sandell Triple	" "
16			17	¼	" "	" "

The images obtained with the 2-inch spectrograph are not in good focus. They are very dense in the region near G, but correctly exposed in the ultra-violet. The spectra extend from λ 3350 to λ 5100. Apparently the maladjustment of focus has produced a linear distortion of the images; and at the edges of several of the spectra, where the direction of the distortion coincides with the direction of the bright lines the focus appears to be quite perfect through the whole length of the spectrum

This suggests that the lens which was a thin one was under some strain in its cell and it accounts for the difficulty experienced in finding the true focus

In the mid eclipse photograph (No 11) the bright lines are fairly well defined at the extreme end of the spectrum and they can be traced in this photograph to λ 3320 All the lines between λ 3340 and λ 3500 can be identified with those shown on the best plate obtained in 1898

The following table gives the wave lengths and identifications of these lines as determined for the spectrum obtained in India In identifying the lines with the elements given in column 4 I received great assistance from Mr L F Jewell who also supplied me with a revised list of wave length values for the solar lines given in column 5

The intensities (column 2) are estimated as follows —

Lines just visible but extremely faint — 0
The strongest lines in the spectrum — 10

Wave length (flash spectrum)	Intensity	Character	Element	Wave length Rowland (solar spectrum)
3326 ±	2	Wave lengths roughly estimated on photograph No 11 of 1900		
3330 ±	2			
3333 ±	1			
3335 ±	2			
3340 0	1½	First line on photograph No 3 of 1898	Cr ? *Ti*	3340 490
3342 3	2		Ti	42 012
3347 0	0	(One measure only)	Ti	46 882
3349 4	4	Long	Ti	49 558
3354 0	2	Faintly extended	Sc	53 875
3358 5	2		Cr	58 649
3361 4	3½	Long	Ti	61 327
3368 3	3	Long	Cr	68 193
3373 0	4	Long	Ti	72 948
3380 4	3	Short	Ti	80 424
3384 0	4	Long	Ti	83 892
3388 1	3	Long	Ti	87 983
3392 1	1½	(One measure only)	Zr ?	92 109
3394 7	3	Long	Ti	94 718
3399 3	1	Short		
3403 45	3	Faintly extended	Cr	3403 404
3405 17	1	Short	Co ?	05 217
3407 32	1	Short	Fe ?	07 597
3408 97	3	Long	Cr	08 911
3410 24	1	Visible on south side only		
3415 01	1	Visible on south side only	Ni	14 911
3421 43	3½	Equal pair long .	Cr	21 353
3422 94	3½		Cr	22 892

Wave-length (flash spectrum).	Intensity	Character	Element	Wave-length, Rowland (solar spectrum)
3425·46	0	One measure only, very short .		
3426·97	0			
3428·73	0			
3430·61	1	Short.	Zr ?	3430·671
3433·54	1	Long . .	*Cr*, Ni	33·453 33·715
3438·40	2	Short	Zr ?	38·376
3440·93	3	Faintly extended . .	Fe, Fe	40·762 41·155
3442·24	1	Long	..	42·112
3444·39	3	Faintly extended .	Ti	44·167
3446·34	2	Ill-defined	Ni	46·406
3452·86	2		Ni	53·039
3456·55	2	Ill defined, short . .	Ti	56·528
3458·58	1		Ni	58·601
3460·53	3	Long . . .	Mn	60·160
3461·68	2	Faintly extended. .	Ti, Ni	61·633 61·801
3463·05	1	Interrupted; very short .	Co	62·950
3464·32	1	Visible on continuous spectrum only	Sr ?	64·608
3465·87	2	Very faintly extended	Co, Fe	65·900 66·015
3467·46	0			
3468·72	0	.. .	Fe	68·821
3471·33	1	Equal pair, short, and interrupted . . .		
3472·58	1		Ni	72·680
3474·28	3	Long.	Mn	74·287
3475·67	2	Short . ..	Fe	75·594
3477·26	3	Long . ..	Ti	77·323
3479·48	1½	Ill-defined, short.. . ..	Zr ?	79·531
3481·20	1½		Zr ?	81·302
3483·08	3	Long	Mn	83·047
3488·86	3		Mn	88·817
3491·16	3		Ti	91·195
3493·22	2	Ill-defined	*Ni*, Fe	93·114 93·618
3494·60	1	Short		
3496·17	3	Long		
3497·82	2	Short		
3499·14	0	Interrupted		
3500·45	1	Short		
3502·80	0	(One measure only)	Co	3502·394
3505·06	3½	Long, equal pair..........	Ti	05·086
3510·96	3½		Ti	10·985

In column 4 the predominating element in a group is put in italics.

The sixteen images of the cusp and flash spectra obtained with the reflecting spectrograph are in good focus throughout.* Each spectrum is 8 inches long, and extends from λ 350 to λ 510. The width corresponding to the sun's diameter is 0.68 inch.

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* The very strong chromosphere arcs, such as H and K, show a faint coma on the more refrangible side. This has since been traced to slight irregular refraction at the base of the 60° prism. The fault is, however, too slight to appreciably affect the definition of any but the strongest lines.

The finer flash spectrum lines in many of the photographs are particularly well defined in the ultra violet.

The first four photographs of the series are much over exposed and fogged and only the stronger chromosphere arcs are visible at the edge of the continuous spectrum. In the succeeding images, the sky illumination becoming much diminished, the bright lines show up clearly on a light background.

In No. 9 the flash spectrum is fully developed in a rift in the continuous spectrum. This rift extends from position angle 140 to 148 and includes a region between 67 and 75 south latitude. The bright lines crossing the rift are beautifully defined throughout the spectrum, and in the ultra violet they can be traced nearly as far as the continuous spectrum. The Fraunhofer lines are well defined upon the continuous spectrum where the latter has not been over exposed, and the whole spectrum in the ultra violet is a mixture of bright and dark lines.

Accurate determinations of wave length will result from the measurement of this negative.

No. 11. This was exposed for 10 seconds at the greatest phase of the eclipse. The continuous spectrum is broken up into five narrow bands, and the flash spectrum lines form long arcs crossing the bands. Most of these arcs extend over nearly 80 of the limb and cover the entire south polar region from latitude 75 on the east side to latitude 28 on the west.

The bright lines on this negative are more strongly impressed than on any of the others and they can be clearly traced up to the end of the continuous spectrum at λ 350. The dark lines of the Fraunhofer spectrum are still traceable on the narrow strips of continuous spectrum.

This negative will give good wave length determinations for all the finer lines between λ 350 and λ 510.

Good images of the flash spectrum are also impressed on photographs Nos. 10, 12 and 13.

General Conclusion.

(1) In its main features the flash spectrum at the south pole of the sun is the same as in low latitudes.

(2) No essential change is shown after an interval of four years, the spectra photographed by Shackleton, in 1896, and those obtained in 1898 and 1900, all appear to be identical so far as it has been possible to compare them.

(3) The flash spectrum, therefore, is probably as constant a feature of the solar surface as is the Fraunhofer spectrum.

With regard to instruments the reflecting prismatic camera has proved to be a most efficient form of spectrograph for eclipse work.

The uniform focus over the entire range of spectrum and the facility with which the adjustment for focus can be effected, are advantages which those who have worked with prismatic cameras will appreciate.

Another important advantage in the use of the reflector is the proximity of the exposing shutter to the plate holder both of which can easily be controlled by one person. There is no signalling between the man at the plates and the man at the shutter.

There is again the advantage that there is no selective absorption of ultra violet rays which occurs in lenses and if the mirror is freshly polished there is no selective reflection for any of the rays which can be photographed.

In concluding I have to acknowledge my great indebtedness to my brother for his untiring devotion to the interests of the expedition throughout. In all the negotiations necessary on arrival in the country he took a leading part and was successful in obtaining the goodwill of every person with whom we came in contact.

The fine series of photographs which we obtained bear witness to his skill in carrying out to the letter the somewhat troublesome arrangements which I had planned for erecting and adjusting the instruments.

"Preliminary Note on Observations of the Total Solar Eclipse of 1900 May 28 made at Santa Pola (Casa del Pleito) Spain."

By RALPH COPELAND Ph.D. F.R.A.S. F.R.S.L.—Read at Joint Meeting of the Royal and Royal Astronomical Societies June 28, 1900. MS received October 1 1900.

I had again the honour of being nominated one of the observers for the Joint Eclipse Committee, the station allotted to me being at Santa Pola, on the south east coast of Spain.

On the 9th May I left Edinburgh and sailed from Tilbury on the 11th in the Orient steamship 'Ortona' accompanied by Mr Thomas Heath, First Assistant at the Edinburgh Royal Observatory, who was going to Santa Pola to observe the eclipse on behalf of the Royal Society of Edinburgh.

My instrumental outfit had preceded me under the care of Mr James McPherson, the experienced mechanician of our Edinburgh observatory. This outfit comprised the 40 foot horizontal telescope of 4 inch aperture previously used in India and Norway, together with a small Iceland spar and quartz prismatic camera, with an effective aperture of 1·8 inch.

At St Pancras Station I had the pleasure of joining Sir Norman Lockyer and his party who, like ourselves were bound for Santa Pola

Early on the 16th we reached Gibraltar where we were met by another member of our Edinburgh party Mr Franklin Adams, from Machrihanish who had most thoughtfully arranged for the transfer of all our eclipse apparatus from the Orubi to H M S Theseus, which the Admiralty had generously placed at the disposal of the Joint Committee We were most cordially welcomed on board the Theseus' by Captain Tisdall who introduced us to his officers and assigned to us our most comfortable quarters

The few days spent on board the Theseus passed most pleasantly With the greatest interest we followed the various forms of drill and were greatly struck by the promptitude and precision with which every order was carried out

On landing at Santa Pola on the afternoon of the 17th we were received with the utmost courtesy by the Alcalde and other Spanish authorities who at once assured us of all possible assistance in the furtherance of our work The interchange of courtesies being over, we at once proceeded to the camp already laid out for Sir Norman Lockyer's party Abundant space had been left for the installation of our apparatus, but on closer examination of the ground we found the subsoil too light and sandy to afford the firm foundation required by our heavy instruments We had therefore to select another site This we found in the upper part of the town in a barley field from which the crop had been gathered a few days before Here the solid rock covered only by a thin layer of soil afforded an ideal foundation for all our apparatus, while the neighbouring walls or houses protected the site from the prevailing winds without unduly obstructing the view

To the south east of the selected spot stood a large barn, which chanced to be vacant in consequence of a law suit, and was therefore called La Casa del Pleito This barn was allotted to us by the ever obliging Alcalde and gave the name to our station It served in the threefold capacity of a store place for our empty boxes, a photographic laboratory and a most welcome retreat from the burning rays of the noonday sun

While our instruments were being landed and carted up on the morning of the 18th we commenced laying out and preparing the necessary foundations for them In this as in all our work, we were most efficiently helped by a detachment of junior officers and men from the "Theseus"

For the first few days there was a good deal of cloud, by night as well as by day, and it was only with difficulty that the exact observations requisite for setting up the 40 foot were secured

Saturday, the 19th, was a red letter day for us, as well as for our countrymen throughout the world With his usual thoughtful care, Mr Franklin Adams, before leaving England, had arranged that a concise daily telegram should be sent to him giving the latest war news. These telegrams were at once communicated to both camps as well as to the Theseus and it is needless to say with what keen interest they were received and discussed. We were preparing for lunch at the comfortable little restaurant where we lodged when the telegram announcing the relief of Mafeking was received. Immediately we all rushed into the entrance hall where we gave three hearty British cheers, greatly to the astonishment of our Spanish friends who were quite at a loss to understand what all the cheering and excitement meant.

By Monday the 21st all our heavier concrete foundations were finished and we had a clear week in which to adjust and test our appliances. The weather had also become much clearer particularly in the afternoon when it was important to check the final adjustments of the long telescope at the hour corresponding to that of the eclipse. Eventually all the adjustments were completed and tested by the 27th on the afternoon of which day we had the satisfaction of seeing the sun's image traverse the plate holder of the 40 foot precisely at the computed rate and at the exact distance from the centre line corresponding to the sun's declination at the time.

On the 26th we received a visit from the Civil Governor of the Province of Alicante who was desirous of seeing our apparatus and satisfying himself that everything possible was being done for our comfort and convenience. On the same day a party of French astronomers came over from Elche to see our camp and compare notes. We much regretted that time did not permit our returning their friendly visit.

Meanwhile Mr Heath and Mr Franklin Adams had erected the equatorial stands to carry their apparatus. Mr Heath was provided with a 6 inch photo-visual telescope by T Cooke and Sons arranged to photograph the corona in the primary focal plane, while Mr Franklin Adams' equipment consisted of a number of cameras several of them of large aperture designed for obtaining pictures of the coronal rays and the sun's surroundings generally. He had also several very accurate thermometers mounted on a suitable screen.

The exact duties of each member of the camp were repeatedly rehearsed in accordance with the beats of a metronome, the indications of which were shouted out by a seaman on the plan devised by Sir Norman Lockyer for regulating the numerous operations at his camp. As most of the observers had already practised at their respective instruments, even the first general rehearsal went off much better than we could have expected. The whole credit of this is due to our naval assistants, who, from being trained to act promptly and in concert, readily appreciated the exact nature of the new duties entrusted to them.

In the night all the plate holders were duly filled and arranged in order

For about a week before the day of the eclipse the closely approximate time of Greenwich mean noon was signalled to us from the ship This proved of the greatest value as it relieved the camps of the necessity of making independent time determinations

From the moment at which the erection of the instruments was commenced four members of the Guardia Civil were told off by the Spanish authorities to watch over the safety of our gear A single wire cord stretched round the area occupied by the instruments served as the line of demarcation within which unauthorised persons were not allowed to come On the day of the eclipse this line was thrown somewhat further back at the suggestion of our Spanish friends

The weather on the momentous 28th was all that astronomers could desire With the greatest care all our apparatus was revised The end of our barn had been smoothed over with stucco so as to present a white expanse some 30 feet in width by 15 feet in height on which to observe the shadow bands The azimuth of this wall was very exactly S 40 W and as mid totality occurred in azimuth 92 10 the position of the wall was very favourable for the observations in question We also put up a white screen some 14 feet square projecting at right angles to the northern end of the wall and whitened the ground in the angle thus enclosed thereby giving three planes on which we hoped the bands might be seen Two officers of the Theseus who for some days previously had practised marking and recording imaginary shadow bands were entrusted with the duty of recording the real bands as they appeared on the white surfaces They were provided with brushes attached to long poles and with pots of coloured wash— blue for the beginning of totality and red for the end

I undertook to observe the first contact with a small telescope of 2 inches aperture

This occurred $10^{s}.4$ before the computed time but the discrepancy caused no surprise as the moon's limb was very rough at the point of contact, and there was the chance that our chronometer time might be out a second or two In view of the very important work before us, no photographs of the partial phase were attempted

I have a note that at twelve minutes before totality the sky began to darken very rapidly, the darkness increasing more and more visibly during the last minutes before the total phase Five minutes before totality at the word ' Stations," everyone took up his assigned post The large crowd of spectators who had collected during the last hour or two pressed closer in to the boundary wire—some of them still expressing their doubts as to whether the eclipse would really be total or not

Eighty three seconds before the computed time of second contact I gave the signal "Start the clock" to McPherson who was in the dark room of the 40 foot. At this moment Mr Franklin Adams gave a few strokes on a large bell, and called out *Silencio!* I must here say that this call was immediately obeyed in the most courteous way by the assembled crowds who maintained a perfect silence until the important phase was over.

One minute before totality at the signal "Chronograph" McPherson registered the position of the moving plate holder. Sixteen seconds before totality when according to Mr Fowler's computation the diminishing crescent should subtend an arc of 90 I gave the signal "Stand by" five seconds before totality corresponding to 55 of a crescent the signal "Ready" was called and at the disappearance of the last glimpse of sunlight I gave the final signal of "Go!"

From this moment the sailor in charge of the metronome announced every fifth second during the first minute and then every second until the seventy fifth, when he called "Stop."

One minute and twenty four seconds after the signal I gave to McPherson the final signal "Chronograph" which he again recorded on the moving plate holder assuring himself at the same time that it was still moving at the regular speed. While I was giving the earlier signals just mentioned I noticed a very interesting feature in the diminishing crescent. When the luminous crescent was reduced to a mere line, an exceptionally brilliant bead of light became detached from the rest continuing to shine like a bright star for perhaps four or five seconds and probably disappearing nearly at the same time as the rest of the crescent. It was doubtless due either to the passage of the sunlight through a very deep valley on the moon's limb or to the interruption of the crescent by a high range of lunar mountains. Whatever its origin it presented an extreme case of the well known phenomenon of "Baily's Beads."

What struck me most both in the late eclipse and in that of 1898, was the sudden transition from the swiftly changing phenomena attendant on the disappearance of sunlight to the steady unvarying aspect of the corona. During the last few minutes of the partial phase all the phenomena are in a state of rapid change, the light decreases in a swift geometrical ratio, the last shred of the sun's limb disappears, the prominences burst into view, and all at once the corona stands before one fixed and relatively unchanging during the whole of totality.

The corona as seen with the naked eye presented a striking resemblance to the pictures of the corona of 1878. Below was a broad double streamer, like the outspread tail of a dove, symmetrical to the sun's equator, while opposite to this was a single large pointed streamer involved in a much fainter dove tail symmetrical to the one below. The spectrum shown by an excellent direct-vision prism about mid totality struck me as being very continuous for I did not see the K 1474 ring In common with all other observers I was struck by the extreme brightness and the red colour of Mercury some 2½° preceding the Sun while near the zenith Venus blazed in the purest white

Turning to the photographic results three successful negatives of the prominences and the corona were obtained with the 40 foot with exposures of 5^{s}0 18^{s}6 and 3^{s}0 respectively On the whole the long exposure gives the best picture in the original negative the light of the great equatorial streamers can be traced to a distance of about one solar diameter from the moon's limb while the detail of the shorter streamers is shown with considerable precision The short exposures naturally bring out the shorter rays that are to some extent lost in the brightness due to the long exposure

There are also twenty four spectrograms taken with a direct vision prism drawn in front of the object glass of the 40 foot Of these two groups of ten each were secured one set as totality was coming on and the other immediately after it had ended They were taken on plates measuring 8 inches by 16 inches so arranged as to be moved transversely in the plate holder between each exposure The height of each spectrogram is 0 3 inch while an interval of ¼ inch is allowed between the different pictures The remaining four spectrograms were taken after totality with the 40 foot acting as a prismatic camera The two earlier ones show H K and other lines extending beyond the continuous spectrum while the two last are of little interest except for finding how long after the end of totality it is possible to obtain useful spectrograms

In developing these plates and in making the copies and slides* from them we had the advantage of the skilful assistance of Mr John Banks photographer of Edinburgh

McPherson's position inside the dark room of the 40 foot gave him the unique opportunity of watching the actual image of the sun's appendages as it imprinted itself on the sensitive films At the time of making the exposure of nearly nineteen seconds at mid totality he describes the picture as comparatively dark—very little of the corona being visible the larger prominences were, however noticed, although they were not nearly so bright as afterwards In the last exposure, near the end of totality, the prominences appeared of a bright flaming red colour, and the picture on the plate was altogether a splendid sight Mr McPherson was watching the prominences under the impression that there were still five seconds to spare, as the time keeper at the metronome was counting seventy and we expected the eclipse to last seventy five seconds, when all at once a sudden increase of brightness took place on the moon's limb—white light seeming to curl over the edge of the moon's disc. Immediately concluding that the total phase was all but over, he let go the cord and closed the shutter. When working with the prism the exposures were far too short to permit of seeing the images.

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* The best of these slides as well as contact copies of the larger negatives, were exhibited at the meeting

The spar camera was in charge of William Slaughter, petty officer of the "Theseus". It could not possibly have been in better hands, for in spite of the lightness of the 3 inch equatorial mounting and the delicate clock movement by which it was carried, he exposed all his plates without deranging the instrument in the slightest degree. Six spectrograms were obtained in all, several of which contain many ultra violet lines belonging to the chromosphere, or possibly to the lower layers of the corona.

Pending the presentation of the report on the shadow bands which is in my hands, but which I should like to supplement by a short computation, I may say that the two officers of the "Theseus," Mr Green and Mr Alexander, succeeded in marking the course of the shadow bands on the vertical wall both at the beginning and at the end of the total phase. As totality came on, the faint rippling bands moved to the right upwards, the direction of motion making an angle of 20 to 30° with the vertical, at the end of the dark phase the motion was in the opposite direction—to the left downwards, the motion in both cases being at right angles to the lines. The lines appeared in short wavy fragments. Quite at the last, a little after the main body of the lines had disappeared, there came a solitary thicker line, more distinct than the rest, and moving less rapidly in a direction inclined 47° to the vertical, but otherwise in the same general direction as the rest of the lines. The wall was photographed, but although the negative shows the red lines distinctly enough, the full blue colour used at the beginning of totality, can scarcely be seen in the photograph. No bands were satisfactorily made out on either of the two other planes.

The warmest thanks of our party are due—to the Admiralty for all the assistance given to us, to the officers and crew of H M S. "Theseus" for their hearty co-operation, which contributed so largely to the success of our endeavours, to the British Vice Consul at Alicante, to the Spanish authorities and to our Santa Pola friends for their untiring courtesy and kindness, and to the "Orient" Steamship Company for their very liberal concession in carrying our bulky impedimenta practically free of cost.

Total Eclipse of the Sun, 1900 May 28. Preliminary Account of the Observations made at Ovar, Portugal. By W. H. M. Christie, C.B., M.A., F.R.S., Astronomer Royal, and F. W. Dyson, M.A., Sec. R.A.S. (Read at Joint Meeting of the Royal and Royal Astronomical Societies, June 28, 1900; MS. received October 18, 1900.)

[Plates 2–5.]

I. General Arrangements.

An expedition to observe the total solar eclipse of May 28 having been sanctioned by the Admiralty, it was arranged, in concert with the Joint Permanent Eclipse Committee, that the Royal Observatory party should take photographs of the corona on a large scale for structural detail and on a smaller scale for the coronal streamers, and should also photograph the spectrum of the flash and of the corona. The programme thus naturally divided itself into two parts, Mr. Christie, assisted by Mr. Davidson, taking charge of the first part, and Mr. Dyson of the second.

The party are much indebted to the Portuguese Government for the liberal arrangements made for the conveyance of the observers and their instruments in Portugal free of all charge to and from their observing station at Ovar, and for the great assistance rendered in erecting the instruments, and for a daily time-signal from the Lisbon Observatory direct to the observing station.

They are also indebted to Mr. Frank Rawes of Oporto for making all arrangements for a suitable observing station at Ovar, and for much thoughtful provision for the comfort and convenience of the observers.

The party further received valuable assistance from Mr. J. J. Atkinson, who went with them from England, and from Mr. Arthur Berry, who joined them in Portugal on May 20; they readily joined in all the work of the expedition, such as the erection of huts, instruments, &c. The party are also indebted to them and to Mrs. Kennedy and to Mr. Rawes for assistance in the observations on the day of the eclipse.

Itinerary.—The observing huts and instruments were sent to Southampton on May 8, with the exception of the 16-inch cœlostat mirror, and two boxes of photographic plates, which were taken with the observers' personal baggage. The observers left Greenwich on the morning of Friday, May 11, sailing from Southampton by the Royal Mail steamship 'Clyde,' and reaching Lisbon about noon on May 14. After an interesting visit to the Royal Observatory at Tapada, Lisbon, on May 15, the observers left for Ovar on the evening of May 16, and arrived there on the morning of Thursday, May 17.

They left Ovar for Lisbon on Tuesday evening, May 29, the day after the eclipse, and left for England by the Royal Mail steamship "Magdalena," on Friday, June 1, reaching Southampton on June 4, and Greenwich on June 5. While at Lisbon, the Astronomer Royal had the honour of an audience with His Majesty the King of Portugal.

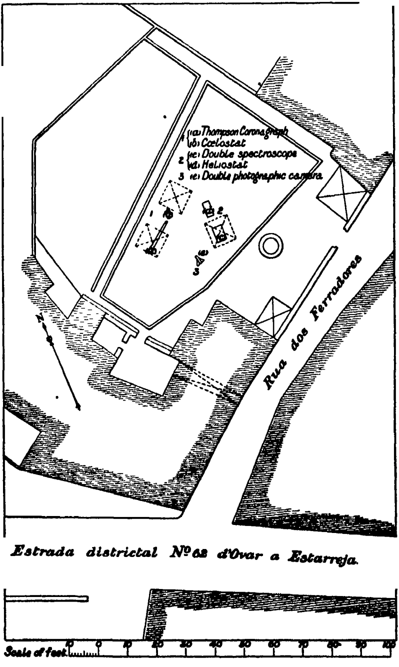
Station.—The station chosen was at Ovar, in Portugal, near the extreme westerly point of the line of totality in Europe, having the advantage of the longest totality

Ovar is a town about twenty miles south of Oporto, on the railway line to Lisbon, it is situated on a sandy plain, which stretches to the sea, the nearest point of the coast being about three miles distant The meteorological conditions of this station proved to be good, the sky being clear on eight of the thirteen days during which the observers were there.

The station occupied by the observers was the garden of Mr. Silveiro's house; its position, taken from the Ordnance map in conjunction with plans of the town, furnished by the Public Works Department, is lat. 40° 51′ 30″ N, and long 8° 37′ 3″ W, and is about $1\frac{1}{8}$ m. from the central line of the eclipse.

Erection and Disposition of Huts and Instruments.—It was found on arrival at the station that the loose sandy soil reached to a depth of at least 18 feet, thus rendering the erection of concrete or masonry piers unsuitable, as well as impracticable. The ground was accordingly cleared on May 17, the day of arrival, and thoroughly rammed; the wet weather which had prevailed previous to the arrival, rendered this the more effectual. On the same day the instruments and materials for the huts were brought from the railway station, half a mile away, in ox-waggons, and partially unpacked. The arrangement of instruments and huts was also roughly marked out.

On the next day, May 18, the huts to cover the instruments were erected, and boxes filled with stones, on which to mount them, were placed in position, The huts were light wooden frames, covered with Willesden waterproof canvas; they were fitted together at Greenwich before starting, and the woodwork marked, so that they were readily fixed up. There were three huts exactly alike structurally, each being 8 feet square and 8 feet high, rising to 10 feet at the gable; the canvas was thrown over the top and sides in two lengths, and tacked down to the woodwork; the ends of the huts were arranged with panels when necessary, which could be removed as required. Two of these huts, without any canvas at the adjacent ends, were bolted together, forming one large hut 16 feet by 8 feet, to cover the coronagraph and 16-inch cœlostat. For observation the bolts were removed, and the hut over the cœlostat moved back a few feet. The spectrographs were in the third hut, and the heliostat supplying them was outside the hut, PLAN OF ECLIPSE STATION AT OVAR, 1900, MAY 28


1 {(a) Thompson Coronagraph
(b) Cœlostat
2 {(c) Double spectroscope
(d) Heliostat
3 (e) Double photographic camera.
1
2
N
S
Rua dos Ferradores
Estrada districtal Nº 62 d'Ovar a Estarreja.
Scale of feet. 10 0 10 20 30 40 50 60 70 80 90 100

and provided with a light canvas cover when not in use. The equatorial with the double tube for the small-scale photographs of the corona was covered with a sheet of waterproof canvas when not in use. The arrangement of the different huts and instruments is shown in the accompanying plan.

The instruments were all erected on May 19, the observers thus having a clear week to adjust them, and to rehearse the observations.

Personnel.—The following list gives the names of those who took part in the observations:

W. H. M. Christie.—Thompson coronagraph. Large-scale photographs of corona.

F. W. Dyson.—Double photographic spectroscope. Spectrum of "flash" and of corona.

C. Davidson.—Double camera. Small scale photographs of corona to show extension.

J. J. Atkinson:—Assisted Mr. Dyson, moving plate-holders and changing plates for the flint prism spectroscope.

A. Berry.—Counted seconds for first half of totality, and then set the heliostat for second contact.

Mrs. Kennedy.—Counted seconds for second half of totality.

Frank Rawes.—Read thermometers during eclipse.

Four Portuguese Soldiers.—Handed plate holders during totality for Mr. Christie and Mr. Davidson respectively.

The following was the method of procedure, which was carefully rehearsed on several occasions previously. The observers were stationed at their instruments, and Mr. Christie watched the diminishing crescent of the sun on the ground glass of his coronagraph. He had a paper scale on which the lengths of the crescent were marked, computed for the intervals 3 mins., 2 mins., 1 min., 45 secs., 30 secs., 25 secs., 20 secs., 15 secs., 10 secs., before totality, at 15 secs. the length of the crescent was 2·64 inches, and at 10 secs. before totality 2·30 inches. Having previously given the signal, "Get Ready," Mr. Christie called out "Ten" at 10 secs. before totality, which was the signal to Mr. Dyson to begin the exposures for the "flash." At totality Mr. Christie again gave the signal (the monosyllable "Tup" was used) and Mr. Berry started a metronome which had been carefully rated to give seconds, and proceeded to count up to 50. Mrs. Kennedy took up the count at 51, and continued counting as far as 100, which had been estimated as 10 secs. beyond totality. While the count was proceeding, exposures at the several instruments were made, as described in the separate reports.

The Day of the Eclipse.—It was quite clear in the early morning, but some light cirrus clouds collected later, causing the observers some apprehensions. There was some light cloud in the sky during totality, but there is no reason to suppose that it interfered seriously with the observations

The first contact occurred at $2^h\ 6^m\ 20^s$ Lisbon Mean Time and was observed by Mr Christie on the ground glass of the coronagraph The time of commencement of the total phase was not accurately noted the duration was observed (by means of a stop watch) and found to be 84½ secs during which the programme detailed below was carried out After totality photographs were taken for orientation The fourth contact was at $4^h\ 36^m\ 13^s$ Lisbon Mean Time There was a good deal of light during totality the diminution of light being similar to that occurring during a heavy thunderstorm in summer The temperature fell about 8° during the eclipse

During totality Mercury and Venus were seen Mercury especially being very brilliant The observers had not much opportunity of observing the attendant phenomena of the eclipse and with the assistance which was kindly given them were only just able to provide adequately for the working of the instruments

II *Photographs of the Corona*

The programme of observations was composed of two distinct parts —

(1.) Photographs of the corona on a large scale to show structural detail

(2.) Photographs on a smaller scale with rapid lenses to show the coronal streamers with the greatest possible extension

(1) *Large-scale Photographs*

(These were taken by Mr W H M Christie)

The instrument used for (1) was the Thompson photographic telescope, with object-glass of 9 inches aperture and 8 feet 6 inches focal length, belonging to the Royal Observatory, in combination with a concave telephoto lens by Dallmeyer of 4 inches aperture and 16 inches focus fitted as a secondary magnifier, to give an image of the Sun 4 inches in diameter, with a field (for full pencils) of 14 inches diameter The total length of the coronagraph was 12 feet—the equivalent focal length being about 36 feet A cœlostat with 16 inch plane mirror (made by Dr Common) was employed to reflect the rays into the coronagraph which was mounted (on boxes filled with stones) so as to point to the mirror at an angle of depression of about 5°, and at an azimuth of about 56° West of South for the day of the eclipse The camera was furnished with five plate-holders to take 15 × 15 inch plates, or for the shorter exposures 12 × 10 inch plates in a carrier

The five slides for photographs of the corona during totality were exposed as below, the exposures being given by the observer with the exposing shutter of the plate holder, and the times noted by him, counting from the commencement of totality

No	Exposure Beginning	End	Duration	Plate		
1	7ˢ	8¾ˢ	1¾ˢ	Lantern	12 in	× 10 in
2	16	22	6	Impress	12	× 10 ,
3	30	50	20	Sandell's Triple coated	15	× 15
4	56	68	12	Special Rapid	15	× 15
5	76	78½	2½	Lantern	12	× 10

As soon as possible after totality a second plate was put in No 5 plate holder and exposed twice on the sun for orientation (with driving clock stopped for 3 min between) the exposure being as short as possible (¼ to ½ sec) and the aperture reduced to 2 inches

Abney squares were put on Nos 1 (twice) and 5 after return home

No 1	300 exposure at 5 ft to standard candle	
No 3	5ˢ and 30	5
No 5	300	3

The plates were all developed after return home hydroquinone dilute being used Nos 1 and 5 unfortunately blistered badly in development especially No 1 though every care was taken the developer being at a temperature of 60 It is to be remarked that other plates developed at the same time under precisely similar conditions were free from blistering No 2 is to a certain extent disfigured with spots on the plate, which, however, do not materially interfere with the coronal detail

No 4 shows fine detail in the polar plumes and coronal streamers extending to nearly a diameter of the Sun from the limb No 3 shows nearly the same

No 5 shows very fine detail in the prominences in the S W quadrant, with gradation of brightness merging into the coronal structure close to the limb, thus showing a continuity between the two phenomena and affording fresh evidence of the association between coronal streamers and prominences, which was indicated in the photographs of the 1898 eclipse

It should be mentioned that the coronagraph was carefully focussed in the same manner as for the eclipses of 1896 and 1898,* by means of the image of an object (gauze net) in the plane of the plate reflected from the plane mirror of the coelostat The focus was thus obtained with great accuracy after two or three trials, and it was found that the field was remarkably flat

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* Monthly Notices R A S, vol 57 p 105; Roy Soc Proc,' vol 64 p 8

(2) *Small-scale Photographs to show Extension of the Corona*

(These were taken by Mr C Davidson)

The double camera, used in former eclipses, was adapted to carry a Dallmeyer rapid rectilinear lens of 4 inches aperture and 34 inches focus, working at f 8 in one half of the tube This lens was lent by the Royal Astronomical Society, the two halves, which had been used separately in former eclipses since 1882, having been reunited for the present eclipse

In the other half of the camera tube there was mounted a new "Unar" lens by Ross, of 2 4 inches aperture and 12 inches focus, working at f/5

Four plate holders, each taking a pair of 16 × 16 cm plates side by side were used during totality, both plates being exposed by a quarter turn of a shutter A fifth plate holder was used to obtain double images of the Sun for orientation as soon as practicable after totality

The double camera was mounted on the equatorial stand of one of the Dallmeyer photo-heliographs, originally made for the Transit of Venus 1874, the middle section of the stand being removed to make it more handy

Both lenses were carefully focussed on star fields, the final adjustment being made by inserting thin metal rings beneath the flange of the lens

The four slides for photographs of extension of the corona were exposed as below, the exposures being noted by the observer, counting from the commencement of totality

No	Exposure			Plate
	Beginning	End	Duration	
1	3s	8s	5s	Sandell Double coated
2	18	48	30	„ Triple-coated
3	57	72	15	„ „
4	80	83	3	Empress

Shortly after totality the fifth slide with Sandell double coated plates was exposed three times on the Sun for orientation at intervals of 3½ mins with the driving clock stopped In this case both lenses were stopped down to their smallest aperture, *i e*, f/64 for the "Unar" lens and f/44 for the Dallmeyer, and the exposure was as short as possible (about ½ sec)

"Abney squares" were put on Nos 2 and 4 after return home.

No 2 30 secs exposure at 5 feet to standard candle.

No. 4 10 „ „ „ „

The photographs with short exposure No 4—3 secs. are the most successful, and show the greatest extension, that taken with the "Abney" lens showing rays which can be traced on the east side to a distance of more than 2° from Sun's centre, and on the west side to a distance of fully 1¾°. This is further than they could be traced visually under the atmospheric conditions at Ovar, where the observers traced them to a distance estimated at ⅓ of distance of Mercury from centre, *i.e.*, 1⅓°.

III. *The Spectroscopic Cameras.*

(By F. W. Dyson.)

Instruments.—The spectroscopes used were two kindly lent by Captain Hills, and employed by him in the Indian Eclipse of 1898, January 22. The details of their adjustments as used at Ovar are as follows:

	Spectroscope No. 1.	Spectroscope No. 2.
Objective	Cooke, achromatic, 4½ in aperture, 6 ft. 2½ in focus	Single quartz lens, 5 in aperture, 4 ft. 7¾ in. focus
Collimator and camera lens	Single quartz lens, 2¼ in aperture, 30 in focus	Single quartz lens, 3 in. aperture, 36 in. focus.
Slit	1¼ in by 0·0011 in	2 in by 0·0012 in.
Prisms	Two dense flint prisms of 60°, 4¼ in base, 2½ in. height	Four double quartz prisms of 60° (each prism being composed of two half-prisms of right- and left-handed quartz), 3¼ in. base, 2¾ in. height.
Prisms at minimum deviation for	H_γ (λ 4340).	H_ζ (λ 3889).

The width of the slits were adjusted by a method given by Mr. Nowall. The third diffraction image of the slit, viewed by putting the eye near the position of the plate, was made to come on the edge of the object-glass by altering the width of the slit, and the slit left at this reading.

The length of the spectrum on the plate for spectroscope No. 1 was 3¼ inches from H_β (λ 4861) to K (λ 3934), and for spectroscope No. 2 2¼ inches from λ 4100 to λ 3500.

Both spectroscopes were mounted horizontally, and were supplied with light by a heliostat furnished with a 12-inch flat mirror.

Erection and Adjustment of Instruments.—As the nature of the ground was unsuitable for brickwork or concrete piers, three of the boxes in which the instruments were carried were filled with stones and the instruments erected on them On one of these—a large clock case, 5 feet long, 2½ feet broad, and 1½ feet high—the heliostat was placed, a light wooden frame holding the two objectives This was covered when not in use by Willesden canvas on a light wooden framework which could be readily lifted on and off The spectroscopes stood on a mahogany table, 5 feet by 4 feet, which rested on the topmost of two boxes The boxes were of such a height that the middle of the slits of the two spectroscopes were respectively half an inch below and half an inch above the centre of the mirror This had to be arranged somewhat carefully as a 5 inch and a 4 inch lens had to be supplied with light by a 12 inch mirror whose normal at the time of the eclipse was inclined more than 40 to the incident and reflected rays The spectroscopes were in a hut, 8 feet square made of Willesden canvas facing north and south and with the north side open when the instruments were in use

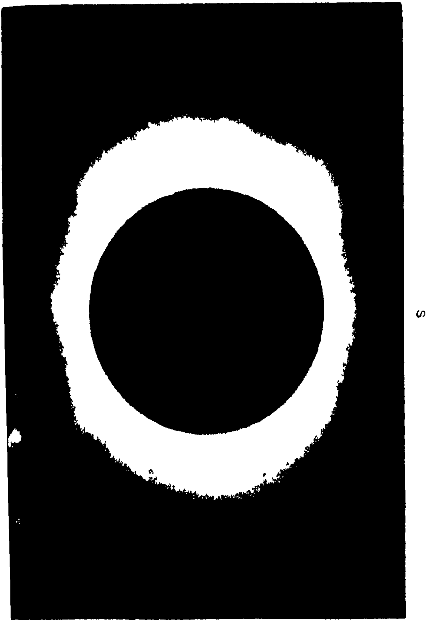
The adjustment of the polar axis of the heliostat was made by means of an attached theodolite the altitude of the axis being first set to the latitude and the azimuth then adjusted by observing the sun's declination at different hour angles from 9^h to 16^h In this way the instrument was readily adjusted till the observed declinations of the sun agreed to within 2 with those of the Nautical Almanac throughout the above range of hour angle The stability of the mounting of the instrument on the sand was quite satisfactory, only very small changes in level occurring, and no perceptible changes in azimuth The only difficulty experienced with the heliostat was in the driving, which is not very satisfactory at large azimuths

Programme of Exposure —The two spectroscopes were adjusted to be as nearly as possible on the sun's limb simultaneously, and the programme of exposures was the same for both The cameras of the two spectroscopes were provided with rack movements, so that a number of exposures could be made on the same plate A flap was arranged so that the exposures for both spectroscopes were made at the same time The programme as arranged with an expected duration of totality of 90 secs was as follows

Ten exposures were made of 1 sec duration beginning 10 secs before totality at about 1 sec apart for the spectrum of the "flash" at the beginning of totality The plates were then changed, and at 20 secs from the beginning of totality the plates were exposed for 50 secs, *i e*, till 70 secs from the beginning of totality for the spectrum of the corona The plates were again changed and the image on the slit moved by Mr Berry by means of the slow motion of the heliostat, and ten exposures of 1 sec duration, beginning at 80 secs after first contact, were given for the spectrum of the "flash" at second contact. The conditions under which the "flash" was photographed, as determined by the circumstances of the eclipse at Ovar and

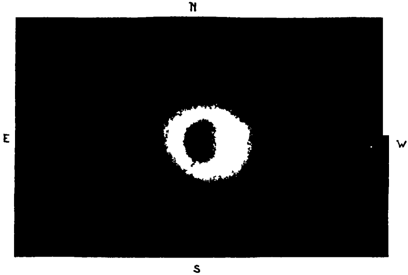
THE SOLAR CORONA

28TH MAY 1900

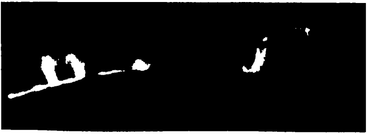


Reduced to [illegible] size from Photograph No. 7 taken at Ovar, Portugal, by W. H. M. Christie. Exposure 15 secs. Thompson Coronagraph Aperture 9 in. Equivalent focus 30 feet

Eclipse of Sun, Ovar, 1900 May 28

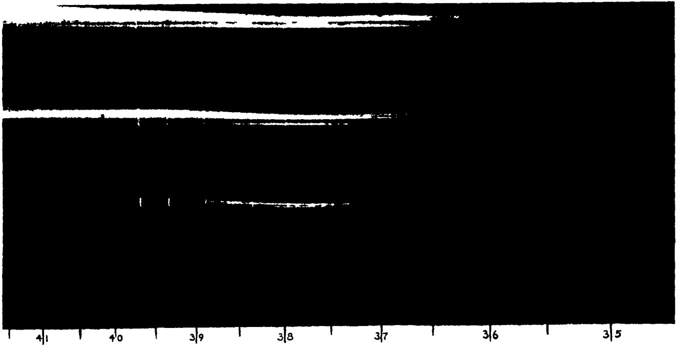


Photograph of Corona obtained with Dallmeyer rapid rectilinear lens of 4 inches aperture (enlarged 1½ times from photograph No 1) [The planet Mercury is shown on the western side of the photograph]



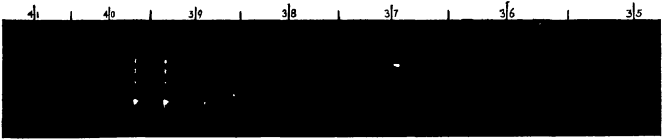
Photograph of Prominences in S W Quadrant (enlarged 2½ times from photograph No 5 taken with the large coronagraph) [The spot with cross rays on the right hand side is a defect in the photographic plate]

ECLIPSE OF SUN, OVAR, 1900 MAY 28

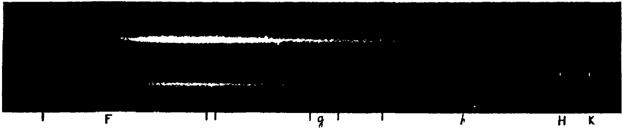


Series of Spectra of Sun's Limb obtained with Quartz Spectroscope near the Commencement of totality (enlarged 2 times)
(Approx. 0s·3, 1s before totality and 1·1s after.)

ECLIPSE OF SUN, OVAR, 1900 MAY 28

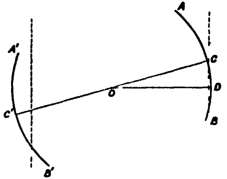


Spectrum of Corona taken with the Quartz Spectroscope (enlarged 1½ ...)



Spectrum of Corona obtained with Flint Spectroscope (enlarged 1½ times)

the instruments used, are shown in the accompanying diagram, which is enlarged about four times



O is the centre of the sun's disc AB is the bright arc as seen 10 secs before totality and is 70 The centre of the arc, C, which is about 1 from the equator is 16 above the point D where the arc is vertical and could be made to touch the slit The slit which is represented by dotted lines cut the bright arc between C and D the horizontal distance between C and D being $\frac{1}{10}$th of an inch The time for the first exposure viz 10 secs before totality was given to the observers by the Astronomer Royal from the length of the rapidly diminishing arc as seen by him on the ground glass of the coronograph This time appears to have been given correct to about 1 sec

The position of the image on the slit was not changed for the spectrum of the corona, which was obtained near the point of second contact

For the "flash' at third contact the slow motion of the heliostat was used, making the sun's image travel in the direction OC of the diagram, the amount of the displacement being determined by watching the sun in the attached theodolite The position of the slit relatively to the bright arc is shown in the second diagram, in this case the slit was not nearly tangential to the sun's limb

The photographic plates used were Ilford Empress for the first flash" photograph with the flint spectroscope and for both the flash photographs with the quartz An Ilford 'Ordinary was used for the second "flash photograph with the flint Cadett 'Lightning plates were used for both photographs of the corona spectrum

Spectrum of the Sun's Limb The series of spectra of the limb show a large number of lines, but they have not yet been examined in detail With the flint spectroscope a spectrum is obtained extending from F to K This is good from F to *h* With the quartz the spectrum reaches from *h* to λ 3300, and is in good definition to about λ 3450 The photographs taken with the quartz spectroscope at the beginning of totality are an interesting series They show a long series of hydrogen lines (26 beginning at *h*), and a large number of iron and titanium lines The difference in behaviour of these two metals is shown in a striking manner the titanium lines, like the hydrogen lines being bright in the whole series of photos beginning 10 secs before totality while the iron lines are reversed in the earlier photographs Titanium lines at wave lengths 3685 30, 3761 46, and 3759 42 are specially bright A reproduction is given of part of this series of photographs (Plate 23)

The Corona Spectra Reproductions of these spectra are given in the accompanying plate (Plate 24) With the flint spectroscope a continuous spectrum is obtained from I to H Light bright lines are distinctly shown stretching right across the continuous spectrum, and several shorter lines in the densest part The line 1474 K is not shown probably because plates specially sensitive in the green were not used The wave lengths of the lines have not yet been determined The positions of the corona lines are indicated on the plate and can be seen in the top band though only faintly

With the quartz spectroscope a continuous spectrum is shown which can be faintly traced as far as λ 3600 Strong bright lines are shown at λ 3987 and λ 3801

November 22, 1900

The LORD LISTER, F R C S D C L, President, in the Chair

Mr John Horne was admitted into the Society

A List of the Presents received was laid on the table, and thanks ordered for them

His Grace the Duke of Northumberland, a member of Her Majesty's Most Honourable Privy Council, was balloted for and elected a Fellow of the Society

In pursuance of the Statutes, notice of the ensuing Anniversary Meeting was given from the Chair, and the list of Officers and Council nominated for election was read as follows —

President —Sir William Huggins, K C B, D C L, LL D

Treasurer —Alfred Bray Kempe, M A

Secretaries.—{ Sir Michael Foster, K.C.B., D.C.L., LL.D.
Professor Arthur William Rucker, M.A., D.Sc.

Foreign Secretary.—Thomas Edward Thorpe, C.B., Sc.D.

Other Members of the Council.—Professor Henry Edward Armstrong, LL.D.; Charles Vernon Boys; Horace T. Brown, LL.D.; William Henry Mahoney Christie, C.B.; Professor Edwin Bailey Elliott, M.A.; Hans Friedrich Gadow, Ph.D.; Professor William Mitchinson Hicks, M.A.; Lord Lister, F.R.C.S.; Professor William Carmichael McIntosh, F.L.S.; Ludwig Mond, Ph.D.; Professor Arnold William Reinold, M.A.; Professor J. Emerson Reynolds, Sc.D.; Robert Henry Scott, Sc.D.; Professor Charles Scott Sherrington, M.D.; J. J. H. Teall, M.A.; Sir John Wolfe Barry, K.C.B.

The following Papers were read:—

I. "Further Note on the Spectrum of Silicium." By Sir J. NORMAN LOCKYER, K.C.B., F.R.S.

II. "On Solar Changes of Temperature and Variations in Rainfall in the Region surrounding the Indian Ocean." By Sir J. NORMAN LOCKYER, K.C.B., F.R.S., and Dr. W. J. S. LOCKYER.

III. "On the Restoration of Co-ordinated Movements after Nerve-crossing, with Interchange of Function of the Cerebral Cortical Centres." By Dr. ROBERT KENNEDY. Communicated by Professor MCKENDRICK, F.R.S.

"Further Note on the Spectrum of Silicium." By Sir NORMAN LOCKYER, K.C.B., F.R.S.

Received October 26,—Read November 22, 1900.

In a previous note* I gave an account of some observations on the spectrum of silicium, and showed the relation which exists between the various groups of silicium lines and certain lines prominent in the spectra of some of the hottest stars.

Further photographs have recently been obtained (with a 3-inch Cooke spectrograph) of the spectrum of silicium bromide in a capillary vacuum tube, and of the spark spectrum between two poles of metallic silicium which were kindly sent to me by Sir William Crookes. In each case the large Spottiswoode coil and plate condenser were used. The spectra extend from about λ 3850 to D, and occupy a length of about 7 inches between those limits. Although all the silicium lines are

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* 'Roy. Soc. Proc.,' vol. 65, p. 449.

Comparison Table of Silicium Lines and their Behaviour in Stellar Spectra

λ	Lockyer			Exner and Haschek (Spark)		Eder and Valenta (Spark)		Intensities in stellar spectra Max = 10			
	Intensity Max = 10										
	Vacuum tube	Spark	Ar	λ	Intensity Max 6	λ	Intensity Max = 10	α Cygni	β Orionis	γ Orionis	ε Orionis
3853 9	< 1	1		3853 62 3854 02	1 1	3854 0	1	3 4	3	1	
3856 1		6		3856 19	5	3856 27	3	6		1	
3862 7	4	4		3862 80	4	3862 75	3	6	5	1	
				3893 46	1						
3905 8	3	10	10	3905 71	5	3905 85	3	4			
				4021 0	1						
4030 0	1	1		4030 1	2						
4089 1	10	2								1	5
4096 9	2	1		4096 8	1						4
4103 2	2	3	4	4103 2	1						
				4103 7	1						
4116 4	6	< 1									6
4128 1	10	10	2	4128 1		4128 2	4	5 6	5	1	1
4131 1	10	10	2	4131 0	6	4131 0	4	5—6	5	1	1
4552 8	10	8		4552 75	3			1	1	4	4
4568 0	8	5		4567 95	1			1	1	3	3
4574 9*	5	3		4574 9	1			< 1	< 1	2	2
				4754 20	1						
5042	5	6	< 1								
5057	10	10	1								

* Previously given as 4575 3 Re-measurement shows this to be an error

common to the two spectra, the relative intensities of the lines differ greatly.

These later photographs show all the lines enumerated in my former communication, with the addition of several fainter lines and a strong double in the green, the latter falling outside the region previously investigated.

Lines in the spark spectrum of silicium have been recorded by Eder and Valenta* and by Exner and Haschek.† A comparison of these with the lines photographed at Kensington is given in the following table.

Better photographs of the spectra of the type stars have been recently obtained, and we are now in a more satisfactory position to trace the various silicium lines through successive stages of stellar temperature. The lines in the spectra of α Cygni, β Orionis, γ Orionis and ϵ Orionis which correspond with lines of silicium are indicated in the table by their intensities.

In my former note the lines were divided into three sets A, B, C, and the behaviour of the different sets in terrestrial and celestial spectra was described. The groupings were then made roughly speaking in order of wave-length. For my present purpose it is important to divide them according to the conditions under which they become prominent lines; this only involves the changing of the order of the groups and involves no interchange of any of the lines from one group to another. Reference was also made previously to a line at λ 3905.8 special to the arc, but as this is not an enhanced line it was not included in any of the sets A, B, C. Hence I now add another group consisting of lines most prominent in the arc spectrum. The lines constituting the various groups will then be as given below, arranged in order of ascending temperature, Group I being the lowest :—

Group IV	4089.1 4096.5 4116.4	Set B of previous note.
Group III	4552.8 4568.0 4574.9	Set C.
Group II	3853.9 3856.1 3862.7 4128.1 4131.1 5042 5057	Set A.
Group I	3905.8 4103.2	

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* Sitz. Kais. Akad. der Wiss. Wien, vol. 107, p. 41.

† Ast. Phys. Jour., vol. 12, p. 48.

The lines in Group I, although appearing in the spark spectrum, are stronger in that of the arc, and therefore cannot be classed as enhanced lines. They are both given by Rowland in his "Table of Solar Wave-lengths" as being coincident with lines in the Fraunhofer spectrum, and may be considered as the lines of silicium which make their appearance at the lowest of the temperatures we are now considering.

It will be seen that only the stronger line of the two is represented in the spectra of the stars included in the table, and that only in α Cygni, which has been placed lowest in order of ascending temperature among those referred to from a previous investigation of lines in its spectrum other than those of silicium.

It does not appear to exist at the higher stages of stellar temperature represented by β, γ, or ϵ Orionis. The absence of the other line from the spectrum of α Cygni may be accounted for by its comparative weakness in the silicium arc spectrum. In α Cygni only the very strongest of the arc lines of iron, manganese, &c., are represented, and then only as very weak lines.

The lines in Group II are either absent from the most recent arc spectrum photographed at Kensington or exist there only as weak lines. The members of this group are prominent both in the vacuum tube spark and in the spark between poles of silicium, but are upon the whole more prominent in the latter spectrum. Considering the first five lines in the group, which are the only ones comparable with the Kensington records of stellar spectra, a glance at the table will show that they are at their maximum intensity at the stage of temperature represented by α Cygni, and decline in intensity as we pass to the higher successive stages represented by β, γ, and ϵ Orionis. At the latter stage some of them have disappeared, and the others are on the verge of extinction. With regard to the remaining two lines of this group, those at $\lambda\lambda$ 5042 and 5057, the position of which cannot be estimated more accurately than to the nearest tenth-metre on account of the diffuseness of the lines, it is extremely probable that if better photographs of that region of the spectra of α Cygni and Rigel were available, lines would be found corresponding to these silicium lines. Keeler has recorded* a line in the spectrum of Rigel at λ 5056, and this is probably identical with the silicium line at λ 5057, which is by far the stronger of the pair.

The lines in Group III occur both in the vacuum tube spectrum of silicium bromide and in the spark spectrum. They appear as a well-marked triplet in the latter, but not nearly so prominently as in the former.

They first make their appearance in stellar spectra in α Cygni, where, however, they can only just be traced. They are a little stronger in β Orionis and are most prominent in γ and ε Orionis in which two spectra they are of about equal intensity

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* 'Ast. and Ast. Phys.,' 1894, vol. 13, p. 489.

The lines in Group IV have never been seen in the spark spectrum of silicium when small coil and small jar capacity are used but with the spark given by the Spottiswoode coil and plate condenser they appear as weak lines They are not like the members of Groups II and III, seen in the spectrum from the bulb when a vacuum tube is used but in that given by the capillary the strongest ones are very prominent and vie in intensity with the lines of Group III

None of them appear in stellar spectra until the level of temperature represented by γ Orionis and in the spectrum of that star only the strongest of the three is with certainty present At the ε Orionis stage however they have developed enormously in intensity and are amongst the most prominent lines in the spectrum

The identity of some of the silicium lines in particular those constituting Group III—with lines in stellar spectra was subsequently but independently confirmed and the results published * by Mr Lunt Assistant at the Royal Observatory Cape of Good Hope

The star the spectrum of which he chiefly considered was β Crucis similar to that of γ Orionis the type star in the Kensington classification

The only enhanced line common to the Kensington and Exner and Haschek's lists which does not appear to be represented in stellar spectra is that at λ 4030 0 It is only a weak line in the spark spectrum and may possibly be due to an impurity though it has not yet been traced to any other origin In the Kensington photograph it is a sharply defined line and unlike the other silicium lines in appearance Exner and Haschek however record it as a very diffuse line

Of the four additional lines given by Exner and Haschek at λλ 3883 46, 4021 0 4103 7 and 4764 20 none appear in any of the Kensington photographs nor are they represented in the spectra of any of the stars included in the discussion With these facts in view it would appear extremely doubtful whether they are really due to silicium

In a former paper On the Chemical Classification of the Stars, † I gave the chemical definition of the various groups At that time only the stronger lines of silicium included in Group II were known and traced through the stellar genera

We are now in a position to revise the chemical definitions interpolating the various groups of silicium lines as they appear in the stellar groups

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* Astrophys Jour vol 11 p 262

† Roy Soc Proc vol 61 p 186

DEFINITIONS OF STELLAR GENERA

Argonian

Predominant —Hydrogen and proto hydrogen

Fainter —Helium, unknown gas (λ 4451, 4457) proto magnesium, proto calcium asterium

Alnitamian

Predominant Hydrogen helium unknown gas (4649 2), silicium (IV)

Fainter —Asterium, silicium (III) proto hydrogen, proto magnesium proto calcium, oxygen nitrogen carbon, silicium (II)

Crucian

Predominant Hydrogen helium, asterium, oxygen nitrogen carbon

Fainter Proto magnesium, proto calcium silicium (III), unknown gas (λ 4649 2) silicium (II) silicium (IV)

Achernian

Same as Crucian.

Proto-metallic lines relatively thick, hydrogen relatively thin

Taurian

Predominant Hydrogen, helium proto magnesium asterium

Fainter Proto calcium, silicium (II) proto iron proto titanium proto chromium nitrogen carbon oxygen

Rigelian

Predominant —Hydrogen, proto calcium proto magnesium, helium silicium (II)

Fainter —Asterium, proto iron carbon, nitrogen proto titanium proto chromium oxygen silicium (III)

Cygnian

Predominant —Hydrogen, proto calcium proto magnesium, proto iron silicium (II), proto titanium proto chromium

Fainter —Proto nickel silicium (I), proto vanadium, proto manganese, proto strontium, iron (arc) helium silicium (III), asterium

Proto metallic lines relatively thin, hydrogen relatively thick.

Algolian

Predominant Hydrogen proto magnesium proto calcium, helium, silicium (II)

Fainter —Proto iron, asterium, carbon proto titanium, proto manganese, proto nickel

Markabian

Predominant —Hydrogen, proto calcium proto magnesium silicium (II)

Fainter —Proto iron, helium asterium proto titanium, proto-manganese proto nickel proto chromium

Sirian

Predominant —Hydrogen, proto calcium, proto magnesium, proto-iron, silicium (II)

Fainter —The lines of the other proto metals and the arc lines of iron, calcium, manganese, silicium (I)

Polarian	***Procyonian***
Predominant — Proto calcium proto titanium hydrogen, proto magnesium proto iron, and arc lines of calcium, iron manganese silicium (I) ***Fainter*** —The other proto metals and metals occurring in the Sirian genus	Same as Polarian
Aldebarian	***Arcturian***
Predominant —Proto calcium, arc lines of iron calcium, manganese proto strontium, hydrogen silicium (I) ***Fainter*** —Proto iron and proto titanium	Same as Aldebarian
Antarian	***Piscian***
Predominant —Flutings of manganese ***Fainter*** —Arc lines of metallic elements	***Predominant*** —Flutings of carbon ***Fainter*** Arc lines of metallic elements

It will be seen that the conclusions arrived at in the former part of the paper as to the different conditions under which the different groups of silicium lines become prominent verify the order in which the stars were placed on a scale of ascending temperatures Thus those stars in which Group I occurs prominently are at the bottom, those in which Groups II and III predominate occupy intermediate positions, and those in which the lines of Group IV are a special feature appear high up in the classification

The photographs of the silicium spectra were taken by Mr Butler Their discussion has devolved upon Mr Baxandall, who has also traced the silicium lines through the stellar spectra, and assisted in the preparation of the paper

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On Solar Changes of Temperature and Variations in Rainfall in the Region surrounding the Indian Ocean

By Sir NORMAN LOCKYER, K C B, F R S, and W J S LOCKYER, M A (Camb) Ph D (Gott)

Received October 26,—Read November 22, 1900

The fact that the abnormal behaviour of the widened lines in the spectra of sunspots since 1894 had been accompanied by irregularities in the rainfall of India suggested the study and correlation of various series of facts which might be expected to throw light upon the subject.

The conclusions already arrived at from bringing together the results of several investigations undertaken with this view may be stated as follows —

(1) It has been found, from a discussion of the chemical origin of lines most widened in sunspots at maxima and minima periods, that there is a considerable rise above the mean temperature of the sun around the years of sunspot maximum and a considerable fall around the years of sunspot minimum.

(2) It has been found, from the actual facts of rainfall in India (during the S.W. monsoon) and Mauritius, between the years 1877 and 1886,* as given by Blanford and Meldrum, that the effects of these solar changes are felt in India at sunspot maximum, and in Mauritius at sunspot minimum. Of these the greater is that produced in the Mauritius at sunspot minimum. The pulse at Mauritius at sunspot minimum is also felt in India, and gives rise generally to a secondary maximum in India.

India, therefore, has two pulses of rainfall, one near the maximum and the other near the minimum of the sunspot period.

(3.) It has been found that the dates of the beginning of these two pulses on the Indian and Mauritius rainfall are related to the sudden remarkable changes in the behaviour of the widened lines.

(4.) It has been found, from a study of the Famine Commission reports, that all the famines therein recorded which have devastated India during the last half-century (we have not yet carried the investigation further back) have occurred in the intervals between these two pulses.

(5) It has been found, from the investigation of the changes in (1) the widened lines, (2) the rainfall of India, and (3) of the Mauritius during and after the last maximum in 1893, that important variations from those exhibited during and after the last maximum of 1883 occurred in all three.

It may be stated at the same time that the minimum of 1888–1889 resembled the preceding minimum of 1878-1879.

(6.) It has been found, from an investigation of the Nile curves between the years 1849 and 1878, that all the lowest Niles recorded have occurred between the same intervals.

(7.) The relation of the intervals in question to the droughts of Australia and of Cape Colony, and to the variations in the rainfall of extra tropical regions generally, has not yet been investigated. We have found, however, a general agreement between the intervals and the rainfall of Scotland (Buchan), and have traced both pulses in the rainfalls of Córdoba (Davis) and the Cape of Good Hope.

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* This period was selected because the Kensington observations of widened lines only began in 1879, and the collected rainfall of India has only been published to 1886.

(8.) We have had the opportunity of showing these results to the Meteorological Reporter to the Government of India and Director General of Indian Observatories, John Eliot, Esq., C.I.E., F.R.S., who is now in England, and he allows us to state his opinion that they accord closely with all the known facts of the large abnormal features of the temperature, pressure, and rainfall in India during the last twenty-five years, and hence that the inductions already arrived at will be of great service in forecasting future droughts in India.

Addendum. Received November 16, 1900.

Since Meldrum and one of us called attention in 1872, to a possible connection between sunspots and rainfall, there has been a large literature upon the subject which it is not necessary for us to analyse; it may be simply stated that in spite of the cogent evidence advanced since chiefly by Meldrum and in later years by Mr. Hutchins,* it is not yet generally accepted that a case for the connection has been made out.

What has been looked for has been a change at maximum sunspots only, the idea being that there might be an effective change of solar temperature either in excess or defect at such times, and that there would be a gradual and continuous variation from maximum to maximum.

At the same time it is possible that the pressure connection first advanced by Chambers is now accepted by meteorologists as a result of the recent work of Eliot.

The coincidence during the last few years of an abnormal state of the sun with abnormal rain in India, accompanied by the worst famine experienced during the century, suggested to us the desirability of reconsidering the question, especially as we have now some new factors at our disposal. These have been revealed by the study, now extending over twenty years, of the widened lines in sunspots, which suggested the view that two effects ought to be expected in a sunspot cycle instead of one.

The Widened Lines.

It will be gathered from previous communications to the Royal Society† that, on throwing the image of a sunspot on the slit of a spectroscope, it is found that the spectrum of a spot so examined indicates that the blackness of the spot is due not only to general but to selective absorption,‡ and that the lines widened by the selective absorption vary from time to time.

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* 'Cycles of Drought and Good Seasons in South Africa,' 1889.

† 'Roy. Soc. Proc.,' vol. 40, p. 347, 1886; vol. 42, p. 37, 1887; vol. 46, p. 385, 1889; vol. 57, p. 199, 1894.

‡ 'Roy. Soc. Proc.' Lockyer, October 11, 1866.

Since the year 1879, the *selective* absorption in spots has been observed for every spot that was large enough to be spectroscopically examined, the method adopted being as follows —

The regions of the spectrum investigated lie between F - *b* and *b* D, and an observation consists in observing the six most widened lines in each of these regions. These lines are then identified on the best solar spectrum maps available and their wave-lengths determined.

An examination of many years' records of these widened lines has shown that at some periods they are easily traceable to *known* elements, while at others their origins have not been discovered, so the latter have been classed as "unknown" lines If we compare these two periods with the sunspot curve as constructed from the measurements of the mean spotted area for each year, it is found that when the spotted area is greatest the widened lines belong to the "unknown" class, while when the spotted area is least they belong to the "known" class

The majority of the lines traced to some terrestrial origin belong to iron, but the lines of other elements, such as titanium, nickel, vanadium, scandium, manganese, chromium, cobalt, &c, are also represented in a less degree.

It is quite likely that some of the "unknown" lines are higher temperature (enhanced) lines of known chemical elements

In our laboratories we have means of differentiating between three stages of temperature, namely, the temperature of the flame, the electric arc, and the electric spark of the highest tension. At the lowest temperature, that of the flame, we get a certain set of lines; a new set is seen as the temperature of the electric arc is reached. At the temperature of the high tension spark we again have many new lines, called enhanced lines, added, while many of the arc lines wane in intensity.

It is found that at sunspot minimum, when the "known" lines are most numerous, the lines are almost invariably those seen most prominent in the arc. Passing from the sunspot minimum towards the maximum the "unknown" lines gradually obtain the predominance As said before, they may be possibly "enhanced lines"—that is, lines indicating the action of a much higher temperature on *known* substances.

Unfortunately the records of enhanced lines at South Kensington, having been obtained from photographs, are chiefly confined to a region of the spectrum not covered by the visual observations of widened lines in sunspot spectra.

We can only point to the evidence acquired in the case of one metal —iron, for which photographs of the enhanced lines, in the green and yellow parts of the spectrum, have been obtained.

This evidence quite justifies the above suggestion, for the enhanced lines of iron can be seen revealing themselves as the number of unknown lines increases

We are therefore quite justified in assuming a very great increase of temperature at the sunspot maximum when the ‘unknown lines appear alone

The curves of the known and unknown lines have been obtained by determining for each quarter of a year the percentage number of known and unknown lines and plotting these percentages as ordinates and the time elements as abscissæ Instead of using the mean curves for all the known elements involved that for iron is employed as it is a good representative of known elements and has been best studied When such curves have been drawn they cross each other at points where the percentage of unknown lines is increasing and that of the iron or known lines are diminishing or *vice versa*

We seem therefore to be brought into the presence of three well marked stages of solar temperature

When the curves of known and unknown lines cross each other that is when the number of known and unknown lines is about equal we must assume a mean condition of solar temperature When the unknown lines reach their maximum we have indicated thus a + pulse or condition of temperature When the known lines reach their maximum we have a − pulse or condition of temperature

The earliest discussion showed that generally speaking the unknown lines curve varied directly and the iron lines curve varied inversely with the spot area curve The curves now obtained for the whole period of twenty years not only entirely endorse this conclusion but enable more minute comparisons to be drawn

The widened line curves are quite different from those furnished by the sunspots Ascents and descents are both equally sharp changes are sudden and the curves are relatively flat at top and bottom The crossings are sharply marked

During the period since 1879 three such crossings have occurred, indicating the presence of mean solar temperature conditions in the years 1881 1886-7 * and 1892 It was expected that another crossing with the known lines on the rise would have occurred in 1897 indicating thereby the arrival of another mean condition of solar temperature but as yet no such crossing has taken place

The following tabular statement shows the years of those crossings, together with the probable dates in brackets of the two previous crossings as determined by the time of occurrence of the preceding sun-spot maximum

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* According to the observations the mean was reached in December 1886 or January 1887

Rise of	Years		
Unknown lines	(1869)	1881	1892
Known lines	(1876)	1886 7	[illegible]

Comparison of Solar and Terrestrial Weather

It has long been known that a cycle of solar weather begins in about lat 32 N and S and in a period of eleven years ends in about lat 5 N and S

Just before one cycle ends another commences The greatest amount of spotted surface occurs when the solar weather changes produced in the cycle reach about lat 16 N and S

It becomes, therefore of the first importance to correlate the times of mean solar temperature and of the + and − heat pulses, with the solar weather cycle in order to arrive at the temperature history of the sun during the period which now concerns us This may be done as follows

Solar cycles										
Lat of spots	19		12		18°		10		19°	
Heat condition	mean	+	mean	−	mean	+	mean	−	mean	+
Years	1869	1870–5	1876	1877–80	1881	1882 6	1886 7	1888–91	1891 2	1892

Connection of the Spots with Prominences

In 1869, when a sunspot maximum was approaching, the prominences were classified by one of us into *eruptive* and *nebulous*, the former showing many metallic lines, the latter the hydrogen and helium lines chiefly This conclusion, which was published in 1870, was subsequently confirmed and adopted by Secchi, Zollner, Sporer, Young, and Respighi

In the same year prominences on the sun's disc were also observed by one of us by means of the C and F lines *

The eruptive prominences, unlike the nebulous ones, were not observed in all heliographic latitudes, but, according to the extended observations of Tacchini and Ricco, had their maxima in the same latitude as the spots. This is especially well shown by the diagrams illustrating the distribution of spots, faculæ, eruptions, and protuberances which are given by Tacchini for 1881 1887 in the 'Memorie della Soc degli Spettroscopisti Italiani,' 1882 - 1888. These curves show in the most unmistakable manner that the spots, faculæ, and eruptive or metallic prominences have their maximum frequency in the same solar latitudes while the nebulous or quiet prominences are more uniformly distributed, and even have maxima in zones where spots are rarely observed This is corroborated by what Professor Respighi many years ago stated

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* Roy Soc Proc, vol 17, p 415

"In correspondence with the maximum of spots, not only does the number of the large protuberances increase, but more than this—their *distribution over the solar surface is radically modified.*"

In his observations, Professor Young found that the H and K lines of calcium were reversed in the chromosphere as constantly as *h* or C, and the same lines "were also found to be regularly reversed upon the body of the sun itself, in the penumbra and immediate neighbourhood of every important spot."[*] This result was confirmed by the early (1881) attempts of one of us to photograph the spectra of the chromosphere and spots, and also by eclipse photographs. In the photographic spectrum, the H and K lines are by far the brightest of the chromospheric lines, and this fact has been utilised by Hale and Deslandres, acting on a suggestion due to Janssen, for the purpose of photographing at one exposure the chromosphere and prominences, as well as the disc of the sun itself, in the light of the K line.

These photographs thus give us in K light the phenomena which one of us first observed by the lines C and F of hydrogen, and thereby present a record of the prominences across the whole disc of the sun as well as at the limb

In such photographs near sunspot maximum, the concentration of the prominences in zones parallel to the equator is perfectly obvious at a glance. Eruptive or metallic prominences are thus seen to cover a much larger area than the spots, so that we have the maximum of solar activity indicated, not only by the increased absorption phenomena indicated by the greater number of the spots, but by the much greater radiation phenomena of the metallic prominences, and there seems little doubt that in the future the measure of the change in the amount of solar energy will be determined by the amount and locus of the prominence area.

Spots are, therefore, indications of excess of heat, and not of its defect, as was suggested when the term "screen" was used for them. We know now that the spots at maximum are really full of highly heated vapours produced by the prominences, which are most numerous when the solar atmosphere is most disturbed.

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[*] 'Catalogue of Bright Lines in the Spectrum of the Chromosphere,' 1872.

The Indian meteorologists have abundantly proved that the increased radiation from the sun on the upper air currents at maximum is accompanied by a lower temperature in the lower strata, and that with this disturbance of the normal temperature we must expect pressure changes. Chambers was the first to show that large spotted area was accompanied by low pressures over the land surface of India.*

Passing, then, from the consideration of individual spots to the zones of prominences, with which they are in all probability associated, it is of the highest interest to note the solar latitudes occupied when the crossings previously referred to took place, as we then learn the belts of prominences which are really effective in producing the increased radiation. The area of these is much larger, and therefore a considerable difference of radiation must be expected

The greater disturbance of certain zones of solar latitude seems to be more influential in causing the + pulse than the amount of spotted area determined from spots in various latitudes.

It is all the more necessary to point this out because the insignificance of the area occupied by the spots has been used as an argument against any easily recognised connection between solar and terrestrial meteorological changes.†

Assuming two belts of prominences N. and S. 10° wide, with their centres over lat. 16 , the sixth of the sun's visible hemisphere would be in a state of disturbance.

Indian Rainfall. S.W. Monsoon, 1877–1886.

It will be clear from what has been stated that our object in studying rainfall was to endeavour to ascertain if the + and – temperature pulses in the sun were echoed by + and – pulses of rainfall. The Indian rainfall was taken first, not only because in the tropics we may expect the phenomena to be the simplest, but because the regularity of the Indian rains had broken down precisely when the widened line observations showed a most remarkable departure from the normal

It was also important for us to deal with the individual observations as far as possible, because it was of the essence of the inquiry to trace the individual pulses if they were found. Hence the S.W. monsoon was, in the first instance, considered by itself, because although Eliot holds that the winter rains (N.E. monsoon) are due to moisture brought by an upper S.W. current,* their incidence is very different and their inclusion might mask the events it was most important to study.

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* 'Abnormal Variations,' p. 1

† "So far as can be judged from the magnitude of the sunspots, the cyclical variation of the magnitude of the sun's face free from spots is very small compared with the surface itself; and consequently, according to mathematical principle, the effect on the elements of meteorological observation for the whole earth ought also to be small" (Eliot, 'Report on the Meteorology of India in 1877,' p. 2).

The first investigation undertaken was the study of the rainfall tables published by the Meteorological Department of the Government of India. These were brought together by Blanford down to the year 1886.† As the widened line observations were not begun at Kensington till 1879, the discussion was limited in the first instance to the period 1877–1886 inclusive, embracing the following changes in solar temperature, occurring, as will be seen, between two conditions of mean solar temperature —

Mean	− pulse.	Mean	+ pulse.	Mean.
1876	1877 1880	1881	1882–1886	1886–1887

Bearing in mind that the intensity of the + pulse may in some measure be determined by the solar disturbances, which for the present are registered by spotted area, it is important to point out that the preceding maximum in 1870 was remarkable for obvious indications of great solar activity.‡

It soon became evident that in many parts of India the + and − conditions of solar temperature were accompanied by + and − pulses producing pressure changes and heavy rains in the Indian Ocean and the surrounding land. These occurred generally in the first year following the mean condition, that is in 1877–8 and 1882–3, dates approximating to, but followed by, the minimum and maximum periods of sun-spots.

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[*] 'Report,' 1877, p. 125.

[†] 'Indian Meteorological Memoirs,' vol. 3.

[‡] "The year 1870 was characterised by an exuberance of solar energy, which is without parallel since the beginning of systematic observations (*i.e.*, since 1825). The number of observed groups far exceeds that of any previous year, and it appears also from a cursory comparison with the maximum year's observations, as recorded by Hofrath Schwabe, that the magnitude of the different groups, as well as the average amount of spotted surface during any period of the year, is unprecedented." ('Monthly Notices,' vol. 31, p. 79, Warren de la Rue, B. Stewart, B. Loewy.)

The table which the authors of this paper give shows that during the year, although observations of the sun were made on 218 days out of the 364, there was no day without spots recorded. In fact, during the whole year no less than 403 new groups of spots were noted, thus showing us that on the average there was more than one new group per diem.

The authors further remark. "A very remarkable feature of the groups observed during the year appears to be their extraordinary lifetime . . . an exceedingly large number of groups completed three, four, and even more revolutions before finally collapsing."

Meldrum, as far back as 1881,* referred to "the extreme oscillations of weather changes in different places, at the turning points of the curves representing the increase and decrease of solar activity"

It was especially in regions, such as Malabar and the Konkan where the monsoon strikes the west coast of India, that the sharpness and individuality of these pulses was the most obvious

One method of study employed has depended upon Chambers's view† that the S.W. monsoon depends upon the oscillations of the equatorial belt of low pressure up to 31° N lat. at the summer solstice The months of rain receipt on the upward and downward swing will therefore depend on the latitude, and these months alone have been considered.

We began by taking elevated stations in high and low latitudes

Leh Lat 34° N 11,500 feet	The 1881 pulse (in July) was the heaviest recorded (1·77 inches) save one in 1882, the rainfall was nearly as high. The pulse felt in 1878 was the highest of all.
Murree Lat 33° N. 6,344 feet	The 1881 pulse (August) is high, but is followed by a higher next year. The 1878 pulse (August) is highest of all.
Newera Eliya Lat 7° N. 6,150 feet	Taking the fall in July and August. The 1881 pulse occurs in 1882, and is highest. Next comes the pulse in 1878.

It must also be stated that if we take the sun-spot maximum, including the period we have chiefly discussed (1877 1886), as normal, it is found that there are variations in rainfall accompanying the preceding and succeeding maxima of 1870 and 1893. This variation indicates the existence of a higher law, but there has not been time to discuss them thoroughly enough to justify any definite statements about them.

The Rainfall of "Whole India."

The next step was to work on a longer base, and for this purpose Eliot's whole India table of rainfall, 1875–1896,‡ embracing both the S.W. and N.E. monsoons, being at our disposal, was studied.

It was anticipated that such a table, built up of means observed over such a large area and during both monsoons, would more or less conceal the meaning of the separate pulses observed in separate localities; this we found to be the case. But, nevertheless, the table helped us greatly, because it included the summation of results nine years later than those included in Blanford's masterly memoir. Predominant pulses were found in 1889 and 1893, following those of 1877–8 and 1882–3. So that it enabled us to follow the working of the same law through another sun-spot cycle, the law, that is, of the mean solar temperature being followed by a pulse of rainfall.

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* "On the Relations of Weather to Mortality, and on the Climatic Effect of Forests."

† 'Indian Meteorological Memoirs,' vol. 4, Part V, p. 271.

‡ 'Nature,' vol. 56, p. 110.

Mean sun	Rain pulse.
1876	1878
1881	+ 1882
1886–7	1889
1892	+ 1893

The main feature of this table is the proof of a tremendous excess of rainfall in 1893 by far the greatest excess of all (percentage variation, + 22). This was far greater than the excess in 1882.

The next remarkable excess occurs in 1878 (percentage variation, + 15).

The pulses in the period stand as follows:—

	Percentage variation	Heat pulse	
Min 1878	+ 15	–	Years after rise of iron lines.
Max. 1882	+ 6	+	Years after rise of unknown lines.
Min. 1889	+ 6		
Max. 1893	+ 22	+	

The variations in the intensities of the pulses of rain at the successive maxima and minima are very remarkable, and suggest the working of a higher law, of which we have other evidence. But, putting this aside for the present, it should be pointed out that even normally we should not expect the same values for the rainfalls in 1882 and 1893, because the amount of spotted area was so different, 1160-millionths of the solar surface being covered with spots in 1883, and 1430 in 1893.

The very considerable variation in the quantity of snowfall on the Himálayas has often been pointed out by the Indian meteorologists. We have, therefore, used the "whole India" curve between 1875 and 1896, to see whether the sun pulses, which we have found to be bound up with the Indian rainfall, are in any way related to the snowfall as might be expected.

The Himálayan snowfall beyond all question follows the same law as the rain, the value occurring at the + and – pulses, as under, being among the highest:—*

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* 'I.M.M.,' vol. 3, p. 235.

	Inches.
−1867–8.	134
+1871–2	110
−1877–8	207
+1882–3	81

From these tables it follows that both in rainfall and snow the quantity is increased in the years of the rise both of the unknown and iron lines.

Other Rainfalls.

Being in presence of pulses of rainfall in India during the south-west monsoon, corresponding with pulses of solar change, it became necessary to attempt to study their origins. We may add that other pulses were traced, especially one in 1875, but the simplest problem was considered alone in the first instance.

The rainfalls at the Mauritius, Cape Town, and Batavia, were collated to see if the pulses felt in India were traceable in other regions surrounding the Indian Ocean to the south and east.

The Mauritius Rainfall.

The rainfall of Mauritius has been obtained by utilising the results that have been published in the Blue Books* issued by the Royal Alfred Observatory since the year 1885. The volume for 1886 gives the yearly total rainfall for every station that was then in use from 1861 up to the year 1885, and these values have been employed; since then, the yearly values have been obtained direct from each of the yearly volumes subsequently published, *i.e.*, to the end of the year 1898.

It was at first thought that the total Mauritius rainfall could be fairly obtained by employing for the period between 1861 and 1886 the means of several stations as given by Meldrum,† and continuing the values from the observations published in the more recent yearly volumes.

It was found, however, that from 1861–1880 the rainfall was obtained from the observations of four stations, while from 1871–1886, the observations from eight stations were employed.

As a study of all the published data showed that more stations might be utilised in determining the total rainfall of Mauritius, it was decided to discuss all the observations afresh, and make use of as many as possible.

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* "Mauritius Meteorological Results."

† 1861–1880. 'Relations of Weather to Mortality, &c.,' 1881, p. 36. 1871–1886. 'Annual Report of the Director of the Royal Alfred Observatory for 1886,' p. 18.

To this end the records of twenty-eight stations, situated in six different districts, were chosen, and the total rainfall for each year obtained It is only natural that the number of rain gauge stations in the early year of 1860 was not so numerous as that of more recent years, the facts may be stated as follows·

Years	Mean yearly rainfall variation from normal.	Number of stations used.	Years	Mean yearly rainfall variation from normal.	Number of stations used.
	Inches.			Inches.	
1861	+26·6	1	1880	−19·3	23
1862	−10·2	4	1881	− 7·3	25
1863	+ 9·6	6	1882	+16·6	25
1864	−12·2	8	1883	+ 1·8	26
1865	+22·6	10	1884	−12·4	25
1866	−18·2	10	1885	− 0·8	26
1867	− 6·6	11	1886	−35·3	26
1868	+27·1	11	1887	− 4·2	27
1869	− 3·3	12	1888	+22·3	26
1870	− 3·6	12	1889	+18·4	24
1871	−18·9	13	1890	+ 1·2	25
1872	− 7·0	13	1891	+ 1·4	26
1873	+10·3	13	1892	+ 5·1	19
1874	+17·4	17	1893	− 7·4	24
1875	+ 3·0	15	1894	− 0·6	24
1876	− 6·5	17	1895	+10·0	21
1877	+31·4	19	1896	+17·6	22
1878	− 3·8	22	1897	−19·6	24
1879	− 5·2	22	1898	− 2·1	24

With regard to the general rainfall of Mauritius throughout the year, it may be stated that on the average the most rainy months are from December to April, both months inclusive.

The months of November and May are those in which the daily rainfall is increasing and diminishing respectively. Sometimes in July or August there is a slight tendency for a small increase.

The Mauritius Rainfall Curve for the period 1877 1886.

In plotting the Mauritius rainfall curve for the period 1877–1886, it was observed that, the curve is of a fairly regular nature, showing alternately an excess and deficiency of rainfall.

The highest and lowest points of the curve will be gathered from the following table :—

Year	Maximum	
	Excess.	Deficiency
1877	31·4	—
1880	—	19·3
1882	16·0	—
1886	—	35·3

Comparing the times of occurrence of the two pulses of rainfall at Mauritius with the times of the crossings of the known and unknown lines, it is found that the Mauritius maximum rainfall of 1877 occurs about a year after the rise of the known lines in 1876. The next Mauritius pulse of rainfall in 1882 follows the succeeding crossing, when the unknown lines are going up, also about a year later.

Comparison of the Mauritius Rainfall with those of Leh, Murree and Newera Eliya for the period 1877–1886.

The most prominent feature of the Mauritius rainfall for this period was the great excess in the years 1877 and 1882.

Both of these pulses have corresponding maxima in the curves for the rainfalls of Leh, Murree and Newera Eliya, the dates of these in all three cases being 1878 and 1882.

The delay of about a year in the effect of the Mauritius pulse being felt in Ceylon and India, is exactly what would be expected if the rain at sun-spot minimum comes from the south, as has been surmised.

The fact that the pulses at Mauritius, Ceylon, and India in 1882 occur simultaneously, is very strong evidence in favour of an origin in the equatorial region itself for the Indian rain at sun-spot maximum. The pulse at maximum in the Indian south-west monsoon may depend to a large extent upon the action of the excess of solar heat on the equatorial waters to the south of India, and not on an abnormal effect on the south-east trade.

We have found that there was a defect of the usual rainfall at Mauritius in 1892–93, and yet the rain supply in India was in excess.

RESULT OF THE COMPARISON OF RAINFALL.

The + and – Pulses.

It seems quite certain that we are justified in associating the 1878 pulse of rainfall during the south-west monsoon in India with the rainfall common to Mauritius, Batavia, and the Cape at that date; that in all cases the rain has been associated with some special condition connected with the south east trade in the Indian Ocean.

The rainfall of Cordoba suggests that the same trade wind in the Atlantic Ocean was similarly affected at the same time.

The Cause of the Tide

Mr Eliot long ago conjectured that the rainfall of India was profoundly modified by events taking place from time to time in the Southern Ocean. In his Annual Summary for 1896 he wrote as follows:

It has apparently been established in the discussion that the variations of the rainfall in India during the past six years are parallel with and in part at least due to variations in the gradients, and the strength of the winds in the south east trade regions of the Indian Ocean. The discussion has indicated that there are variations from year to year in the strength of the atmospheric circulation obtaining over the large area of Southern Asia and the Indian Ocean, and that these variations are an important and large factor in determining the periodic variations in the rainfall of the whole area dependent on that circulation, and more especially in India. It has also been indicated that these variations which accompany and are probably the result in part of abnormal temperature (and hence pressure) conditions in the Indian Ocean and Indian monsoon area may be in part due to conditions in the Antarctic Ocean which also determine the comparative prevalence or absence of *icebergs* in the northern portions of the Antarctic Ocean.

We have begun an investigation into the pressure changes which have been recorded in this region, but it will be some time before it is finished. The idea underlying the inquiry is that the reduced solar temperature may modify the pressure so that the high pressure belts south of Mauritius may be broken up and thus allow cyclonic winds from a higher latitude to increase the summer rains, as they certainly were increased at the normal minima of 1877 and 1888.

It has been shown that the pulse is felt in India about a year later than it commences action in the Southern Ocean, while in some cases the + pulse is felt almost simultaneously in India and at the southern stations.

The Rainfall at the Cape, Batavia and Cordoba, for the period 1877—1886

Each of the curves (fig. 1) illustrating the rainfall for the Cape and Cordoba (Arg.) for this period shows two prominent maxima in the years 1878 and 1883; these correspond nearly with the + and − pulses of solar temperature Comparing them also with the Bombay and Mauritius curves for the same period, it is found that the pulses indicated at Bombay occur simultaneously with those of 1878 and 1883, but in the case of Mauritius the effect of each of the pulses is felt about a year or so earlier, namely, 1877 and 1882

The rainfall curve for Batavia for this period has its most prominent maximum in the year 1882, like that of Mauritius, thus preceding by a year the pulse felt at the Cape, Córdoba, and Bombay in 1883.

The Time Conditions of the Pulses.

The various curves which we have drawn for the purposes of study have been compiled from yearly means, and so far, in these curves the rainfall in months has not been considered. That will have to come later. Hence if the rainfall which most influences the yearly mean occurs in the last three months at one place, and in the first three months of the next year at another, they are shown as being a year apart, whereas they have actually been continuous.

With regard to the travel of the pulses over large areas under the influence of the S.E. trade, it may be gathered from the pressure charts that the + and conditions of pressure are apt to lie over the centres of land and water areas, and not generally over coast lines. In the case of water surfaces, the effect of a sudden change in the solar radiation on the pressure might be expected to be felt not at the point where the pressure is least or greatest at the time, and of the most general type, but where the equilibrium is most unstable On the other hand, more time would be required for the new pulse to establish itself where the conditions are more complicated

Hence we should expect the pulses to be felt first in the eastern part of the Southern Ocean, and this seems generally to be the case. Thus after the mean solar temperature of 1876, the – pulse was felt first at Mauritius, then in India, and the Cape. After the mean of 1881, the + pulse was felt first at Mauritius, then in India, and the Cape. Cordoba felt both pulses in the same year as India and the Cape.

Subsidiary Pulses.

In a normal sun-spot curve we find a sharp rise, generally taking three or three-and-a-half years, to maximum, and a slow decline to minimum, on which the remaining years of the cycle are spent.

The curve on the upward side rises generally regularly and continuously, on the downward portion the regularity of the curve is very often broken by a "hump" or sudden change of curvature. There has not yet been a complete discussion of the number and character of the prominences associated with the spots during the cycle, we have found however that the hump in the sun spot curve in 1874 was accompanied by a remarkable increase in the number of eruptive prominences

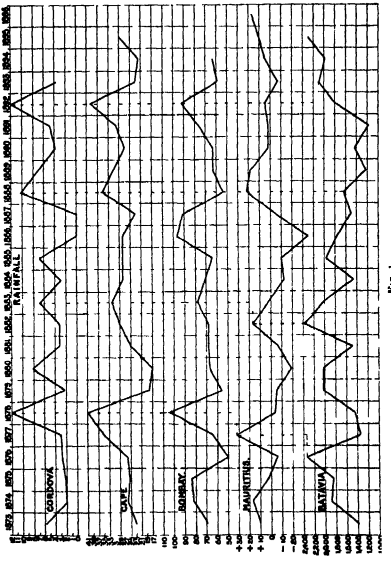

Fig 1

We have already referred in discussing the Indian rainfall, to a remarkable intensification of the south west monsoon in 1874 75, the effect of which is especially noticeable in the rainfalls of the Konkan and North west Provinces, and we have come to the conclusion that we must consider all these events as due to a common cause—that is, to

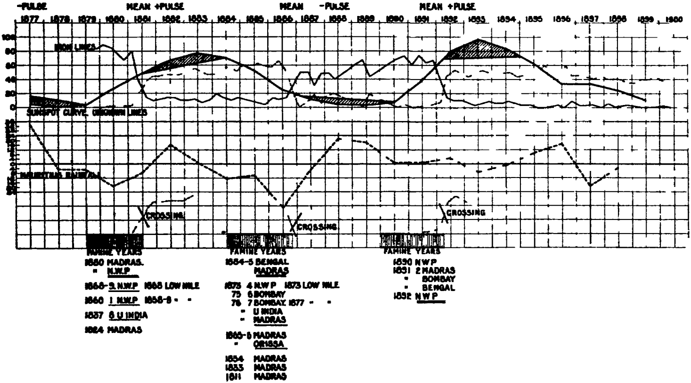
-PULSE
MEAN +PULSE
MEAN -PULSE
MEAN +PULSE
1877 1878 1879 1880 1881 1882 1883 1884 1885 1886 1887 1888 1889 1890 1891 1892 1893 1894 1895 1896 1897 1898 1899 1900
100
80
60
40
20
0
IRON LINES
SUNSPOT CURVE UNKNOWN LINES
MAURITIUS RAINFALL
CROSSING
CROSSING
CROSSING
FAMINE YEARS
1880 MADRAS.
" N.W.P
1868-9 N.W.P 1868 LOW NILE
1860 1 N.W.P 1858-9 " "
1837 8 U INDIA
1824 MADRAS
FAMINE YEARS
1884-5 BENGAL
MADRAS
1873 4 N.W.P 1873 LOW NILE
75 6 BOMBAY
76 7 BOMBAY 1877 " "
" U INDIA
" MADRAS
1865-6 MADRAS
" ORISSA
1854 MADRAS
1833 MADRAS
1811 MADRAS
FAMINE YEARS
1890 N W P
1891 2 MADRAS
" BOMBAY
" BENGAL
1892 N W P

FIG 2

a subsidiary solar pulse We propose to return to this subject in a subsequent communication after inquiries have been completed relating to 1885 86 and 1896 97

[illegible]

There will obviously be intervals between the ending of one pulse and the beginning of the next unless they either overlap or become continuous

The + and pulses to which our attention has been chiefly directed, are limited in duration and when they cease the quantity of rain which falls in the Indian area is not sufficient without water storage for the purposes of agriculture they are followed therefore by droughts and at times subsequently by famines (fig 2)

Taking the period 1887 89 we have

Rain from pulse	78 79 (part)
No rain pulse	79 (part) 80 (central year) 81 (part)
Rain from + pulse	81 (part) 82 83 84 (part)
No rain pulse	84 (part) 85 } (central years) 86 } 87 (part)
Rain from pulse	87 (part) 88 89

The duration of these + and pulses of rainfall was determined in the first instance by the Mauritius rainfall which shows both pulses and later from the Malabar rainfall, which perhaps shows the effect of the south west monsoon in its greatest purity

All the Indian famines since 1836 (we have not gone back further) have occurred in these intervals carried back in time on the assumption of an eleven year cycle

The following tables show the result for the two intervals —

The Interval between the Pulses, taking 1880 as the Central Year, on the Upward Curve.

1880, Madras famine.
N W P. famine.
1880 - 11 = 1869, N.W.P. famine (1868 9).
1869 11 – 1858, N.W.P. famine (1860)
1858 - 11 – 1847.
1847 - 11 - 1836. Upper India famine (1837 8).
(Great famine.)

The Interval between the Pulses, taking 1885-6 as the Central Years, on the Descending Curve.

$$1885\text{-}6\left\{\begin{array}{l}\text{Bengal famine}\\ \text{Madras famine}\end{array}\right\}(1884\text{-}5).$$

1885 6 11 – 1874 5, N.W.P..famine (1873-4).
Bombay famine (1875-6).

$$\left.\begin{array}{l}\text{Bombay famine}\\ \text{Upper India famine}\end{array}\right\}(1876\ 7).$$

$$1874\text{-}5 - 11 = 1863\ 4,\ \left.\begin{array}{l}\text{Madras famine}\\ \text{Orissa famine}\end{array}\right\}(1865\ 6)$$

1863-4 - 11 – 1852 3, Madras famine (1854).

It is clear from the above table that if as much had been known in 1836 as we know now, the probability of famines at all the subsequent dates indicated in the above tables might have been foreseen

The region of time from which the above results have been obtained extended from 1877 to 1886 The next table will show that if the dates, instead of being carried back, are carried forward, the same principle enables us to pick up the famines which have devastated India during the period 1886 97.

Same intervals, going Forward.
1880.

+11 1891, N W.P. famine (1890).

$$\left.\begin{array}{l}\text{Madras famine}\\ \text{Bombay famine}\\ \text{Bengal famine}\end{array}\right\}(1891\text{-}2).$$

1885-6.
+11 1896 7, General famine.

This result has arisen, so far as we can see, from the fact that the + and - pulses included in the period 1877-1886 were normal; that is, were not great departures from the average.

Nile Floods

After we had obtained the above results relating to the law followed by the Indian famines we communicated with the Egyptian authorities with a view of obtaining data for the Nile Valley

We have since found however from a memorandum by Eliot * that Mr Wilcocks in a paper read at the Meteorological Congress at Chicago, remarked that famine years in India are generally years of low flood in Egypt

It remains only for us therefore to point out that the highest Niles follow the years of the + and pulses Thus

1871 one year after + pulse 1870
1876 two years after subsidiary pulse of 1874
1879 two years after pulse 1877
1883 4 one and two years after + pulse 1882
1893 1 after + pulse 1892 (India excess rainfall 1892 3 4)

[illegible] of 1899

When in a sunspot cycle the solar temperature is more than usually increased the regularity of the above effects is liable to be broken as the advent of the pulse is retarded

This as we have already pointed out is precisely what happened after the abnormal + heat pulse of 1892 following close upon the condition of solar mean temperature

The widened line curves instead of crossing according to the few precedents we have in 1897 or 1898 have not crossed yet that is the condition of ordinary solar mean temperature has not even yet been reached

We have shown that as a matter of fact in a normal cycle India is supplied from the Southern Ocean during the minimum sun spot period and that this rain is due to some pressure effect brought about in high southern latitudes by the sun at temperature

As the temperature condition was not reached in 1899 as it would have been in a normal year the rain failed (fig 3)

We may say then that the only abnormal famine recorded since 1836 occurred precisely at the time when an abnormal effect of an unprecedented maximum of solar temperature was revealed by the study of the widened lines

We desire to tender our acknowledgments to Dr Buchan, F R S and Mr Shaw F R S, for their kindness in so promptly replying to our appeal for rainfall tables We wish also to thank Mr H Shaw one

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* Forecast of S W Monsoon rains of 1900

of the teachers in training at the Royal College of Science for assistance in bringing together rainfall data and plotting numerous curves

Fig. 3

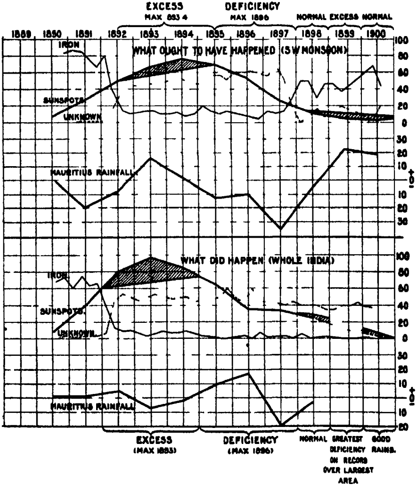

Table showing the Occurrence of the + and − Rainfall Pulses in other Parts of the World.

	+ 1870.	− 1877	+ 1882.	− 1886	+ 1892.
Batavia	—	1876	1882	1883	(?)
Mauritius	—	1877	1882	1888	1892
Cathemenburg (Russia)	—	1877	abs.	1887-8	1892
Scotland	—	1877	—	—	1892
Copenhagen	1872-3	1877	1882	1888	1891
Adelaide	1870	1877	1883	1889	1892-3
Tiflis	1870	1878	1881	1886-90	1893
Archangel	1872	1878	1881-2	1887-8	1892
Brussels	—	1878	1882	1888	1892
Hobart Town	—	1878	1882	1887	1893
*Malabar	1871	1878	1882	1888	1892
Toronto	1870	1878	1888	1886	1893
Córdoba (Arg.)	—	1878	1883	1888	1892
Cape	—	1878	1883	1888	1892
Java	1872	1879	1882	—	1893
Barnaul (Russia)	1872	1879	1882-3	1887	1894
St. Petersburg	1871	1879	1883	1888-9	1893
Nile	1871	1879	1883-4	—	1893-5

* For comparison

"On the Restoration of Co-ordinated Movements after Nerve Crossing, with Interchange of Function of the Cerebral Cortical Centres."

By Robert Kennedy, M.A., D.Sc., M.D., Assistant Surgeon to the Western Infirmary, Glasgow. Communicated by Professor McKendrick, F.R.S. Received October 11,—Read November 22, 1900.

(Abstract.)

I.—*Experiments on Nerve Crossing.*

The experiments on nerve crossing were undertaken in order to ascertain whether, after division and cross union of the entire nerve supply of two antagonistic groups of muscles, the animal can regain the power of performing voluntary co-ordinated movements with the affected muscles, and also to ascertain the effects on the cerebral cortical centres affected by the crossing.

The object of this was to ascertain if the organism has the power to compensate for a change whereby nerve centres are brought into connection with peripheral endings, not by nature belonging to them.

The experiments were made on the right fore-limb of dogs, and

were five in number. The first four were of the same kind, and consisted in uniting the central segments of the divided musculo-cutaneous, median, and ulnar nerves to the peripheral segment of the divided musculo-spiral, and *vice versâ*. Thus the entire supply of the flexor muscles of the forearm was crossed with the entire supply of the extensor muscles. The musculo-cutaneous was included in the crossing, as it sends a communicating branch to the median at the elbow, which branch may contain efferent fibres to muscles.

The nerves were divided above but near the elbow joint, and the two points of union were therefore situated one on the outside and one on the inside of the limb, with a bulky muscle between them, which prevented any possibility of confluent reunion of all the divided ends.

One of the experiments (Exp I) was a failure on account of the wound becoming septic, but in the remaining three (Exp. II, III, IV) the animals regained almost completely the power of making voluntary co ordinated movements of the limb. Thus the leg was used constantly and perfectly in walking and running, and in performing such co-ordinated movements as giving the paw on request, using the paw to hold a bone while gnawing, &c The recovery of function commenced about the 30th day after the operation, and was almost perfect from the 15th to the 90th day.

The physiological examination showed that the nerves which had been crossed had united as they had been placed without one point of union communicating with the other, and that the flexor muscles were thus entirely supplied by the musculo-spiral, and the extensor muscles entirely by the median, ulnar, and musculo-cutaneous.

In two of the experiments (Exp. II and III) the musculo-spiral stimulated above the seat of union gave flexion of the paw, and no movement in the extensor muscles, while stimulation of the central segments of the musculo-cutaneous, median, and ulnar gave extension of the paw, and no movement in the flexor muscles. Stimulated on the cerebral cortical centres of the sigmoid gyrus, it was found that on the left hemisphere the centre which normally gives on stimulation flexion of the paw, gave on the contrary extension, and no movement whatever in the flexor muscles. Stimulation of the centre, normally associated with extension of the paw, gave in one of the animals pure flexion of the paw and no contractions of the extensor muscles (Exp. III), while in the other animal the flexion centre was found to lie in the normal extension area, but pure flexion could not be obtained free from extension movements (Exp. II).

In the other experiment (Exp. IV) the results of stimulation were somewhat obscure. Stimulation of the central segments of all four nerves gave contractions in the extensor muscles and no contractions in the flexors. Yet the flexors were perfectly healthy in appearance and possessed normal irritability to faradic stimuli. Stimulation of the centres on the left sigmoid gyrus showed that the flexion centre had become an extension centre but no flexion centre could be discovered

In these experiments (II III IV) the centres on the right side of the brain were normally placed

In all the experiments the irritability of the centres on the left side of the brain was increased rather than diminished

In addition to these experiments on nerve crossing there was also an experiment made on a dog to ascertain if the fact of crossing the nerves delayed the functional recovery beyond what would be expected merely as a result of nerve section In this experiment the same nerves were divided but were immediately reunited as accurately as possible The result was that the course of recovery of function was not materially different from the course in the experiments on nerve crossing

The physiological examination showed that the nerves had united well, and regained their normal irritability and conductivity and that the muscles of the limb were healthy Examination of the cerebral cortical centres showed that they were not well defined but neither were they on the sound side in this animal

II *Junction of the Peripheral Segment of the Divided Facial Nerve with the Trunk of the Spinal Accessory Nerve for the Treatment of Facial Spasm in a Woman*

The experiments on dogs having shown that nerve crossing was followed by recovery of co-ordinated function the following operation was undertaken for the treatment of facial spasm in a woman Faure and Furet had already suggested utilising the branch of the spinal accessory to the trapezius for the supply of the face in the case of paralysis of the facial nerve and Faure* had put the operation in practice but without success In the following case the patient had suffered for ten years The right side of the face was incessantly twitching the angle of the mouth being permanently drawn up and the eyelids half closed The condition had been under treatment at different periods, but without any success Rather the condition got worse

On May 4, 1899, the facial nerve was divided close to its exit from the aqueduct of Fallopius and grafted on to the trunk of the spinal accessory, just as the latter nerve emerges from under the posterior belly of the digastric muscle The digastric situated between the central end of the facial nerve and the junction with the spinal accessory prevented any reunion of the nerve

Immediately after the operation, the right side of the face was in a condition of complete paralysis, and it remained in this condition for

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[*] Faure "Traitement Chirurgical de la Paralysie Faciale par l'Anastomose Spino-faciale," 'Revue de Chirurgie' vol 18 (1898), p 1096

some time the muscles losing their faradic irritability In course of time gradual improvement showed itself heralded first by recovery of faradic irritability in the muscles The earliest indications of improvement were shown in the orbicularis palpebrarum which began to recover faradic irritability and movement thereby enabling the eye to be slightly closed about the 18th day The movement of the muscle on stimulation with the faradic current was however so slight, that there was a possibility of error and the slight voluntary movement might have been due simply to the relaxation of the levator palpebræ By the 49th day however, there was no doubt as the contractions to faradic stimulations were well marked and the palpebral fissure could be voluntarily closed one half

By the 141st day the faradic irritability of the other muscles began to be recovered and by the 155th day the faradic current gave on applying the electrode over the junction between facial and spinal accessory, strong contractions in all the muscles of the face

Improvement gradually continued and on August 17 1900, about fifteen months after the operation the condition was as follows She experienced no difficulty on account of the condition of the face There was no return of the spasmodic condition The conjunctiva of the right eye was quite normal, there was no increased lachrymal secretion and she never was troubled with dust getting into the eye winking was perfectly efficient She could shut the eye completely although not so tightly as in the case of the sound eye The orbicularis palpebrarum also contracted well to reflex stimuli The right side of the brow could be wrinkled to a very slight degree only and movements could be made in the cheek and mouth although they could not well be co ordinated The labial letters could be perfectly pronounced and the buccinator was efficient to prevent accumulation of food between cheek and gums while eating

There was no atrophy of the side of the face, and in repose there was no appearance of facial paralysis the muscles having regained their tonus and the normal sulci being well marked

There was evidence of want of power over the face in the difficulty of raising the eyebrow, or of making a circular aperture with the mouth in whistling or blowing The muscles, however, of these parts were perfectly sound, as the faradic current gave perfectly normal reactions, both when applied directly to the muscles and when applied to the motor point of the nerve This motor point lay about 2 cm lower down than normally, *i e*, over the junction of the facial and spinal accessory

The reactions and movements of the trapezius and of the sterno mastoid were normal

A curious effect resulted when the arm was suddenly thrown up, for the face at the same time was thrown into contractions, owing to the impulses intended for the trapezius being directed to the face If the arm was continued held up these contractions of the face passed off

III —General Conclusions

1 In the fore limb of the dog the nerve supply of the flexor muscles may be crossed with that of the extensor muscles with the result that despite the altered innervation the animal regains as before, the power of performing voluntary co-ordinated movements of the limb

2 The fact of crossing the nerves does not add materially to the time which would be required for recovery of function of the limb if the same nerves were simply divided and reunited by suture as accurately as possible

3 The result of crossing the nerve supply of antagonistic groups of muscles is that the nerve centres which formerly innervated the one group now serve for the other group and this alteration extends to the cerebral cortical centres which become interchanged in position and retain their mutability

4 The cerebral cortical centres which have been made to interchange their positions by the crossing are able in response to the will, to emit impulses which can call forth in the new peripheral terminations movements in perfect co-ordination

5 In man the facial nerve may be detached from the facial centre attached to the spinal accessory nerve and the facial muscles thus innervated by the spinal accessory centre with the result that co-ordinated movements of the face, both voluntary and reflex are at least in part restored

6 In the case of reunion of a divided nerve it is not necessary to suppose that regeneration restores the old paths for the nervous impulses since if new paths are formed by the imperfect co-adaptation of the divided nerve ends with the result of altering the connections between central nerve cells and peripheral endings, the organism has the power of compensating this alteration

7 In the case of paralysis of a muscle or group of muscles, if the nerve supplying the affected muscle or muscles is grafted on to a neighbouring efferent nerve supplying muscles which are healthy, it is probable that the affected muscle or group of muscles, if not already destroyed by degenerative process will regain its normal function

November 30, 1900.

Anniversary Meeting.

The LORD LISTER, F.R.C.S., D.C.L., President, in the Chair.

(A full Report of the Anniversary Meeting, with the President's Address and Report of Council, will be found in the 'Year-book' for 1901.

The Account of the Appropriation of the Government Grant and of the Trust Funds will also be found in the 'Year-book.')

December 6, 1900.

Sir WILLIAM HUGGINS, K.C.B., D.C.L., President, in the Chair.

His Grace the Duke of Northumberland was admitted into the Society.

A List of the Presents received was laid on the table, and thanks ordered for them.

The President announced that he had appointed as Vice-Presidents for the ensuing year—

The Treasurer.
The Astronomer Royal.
Lord Lister.
Mr. J. J. H. Teall.

The following Papers were read:—

I. "The Histology of the Cell Wall, with Special Reference to the Mode of Connection of Cells." By WALTER GARDINER, M.A., F.R.S., and A. W. HILL, B.A. Part I.—"The Distribution and Character of 'Connecting Threads' in the Tissues of *Pinus sylvestris* and other Allied Species." By A. W. HILL.

II. "On the 'Blaze Currents' of the Frog's Eyeball." By Dr. WALLER, F.R.S.

III. "On a Bacterial Disease of the Turnip (*Brassica napus*)." By Professor M. G. POTTER. Communicated by Sir M. FOSTER, Sec. R.S.

IV. "The Micro-organism of Distemper in the Dog, and the Production of a Distemper Vaccine." By Dr. S. MONCKTON COPEMAN. Communicated by Sir M. FOSTER, Sec. R.S.

V. "On the Tempering of Iron hardened by Overstrain." By JAMES MUIR. Communicated by Professor EWING, F.R.S.

The Histology of the Cell Wall, with special reference to the Mode of Connection of Cells.* By WALTER GARDINER, M.A., F.R.S., Fellow and Bursar of Clare College, Cambridge, and ARTHUR W. HILL, B.A., Scholar of King's College, Cambridge.

PART I.

The Distribution and Character of Connecting Threads in the Tissues of *Pinus sylvestris* and other Allied Species. By ARTHUR W. HILL, B.A., Scholar of King's College, Cambridge.

Received July 17,—Read December 6, 1900.

(Abstract.)

The research with which this paper is concerned was undertaken with a view of ascertaining to what extent "connecting threads" are distributed throughout the body of any given plant, and for this purpose the endosperm and the various tissues of the hypocotyl, cotyledons, and root of the young seedling of *Pinus pinea*, and of the adult stem, leaf and root of *Pinus sylvestris*, were examined.

The results show that the presence of such threads can be readily demonstrated in the case of all cells in which the wall retains its cellulose or mucilaginous character, and that in such young tissue as the growing point of the root all the cells are provided with connecting threads. When the lignified or suberised condition has supervened it is difficult or impossible to identify threads, though even in such cases threads may be recognised in certain of the very young elements.

In *Pinus pinea* the tissue of the endosperm, as also that of the germinating seedling, is well connected by threads.

In the cotyledon the absorptive side next the endosperm (corresponding to the lower side of the leaf) shows a certain histological distinction in that the walls of the cells, both of the epidermis and of the subjacent parenchyma, are more richly provided with threads than are the similar tissues of the upper side. No threads, however, occur

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* For the preliminary communication on this subject, see Gardiner, "The Histology of the Cell Wall, with special reference to the Mode of Connection of Cells," 'Roy. Soc. Proc.,' vol. 62, 1897.

in the outer or free walls of the epidermis so that diffusion only (as opposed to direct transference) can take place between the cell contents of the endosperm and those of the cotyledon.

In the stomata of the cotyledon threads have been seen in a few cases connecting the guard cells with the epidermal cells.

The parenchymatous tissue all over the seedling plant shows connecting threads of a similar character. In the end walls of the cells they occur irregularly scattered but in the lateral walls they are usually in isolated groups mainly in consequence of the growth in length which these walls have undergone and are also situated in shallow pits.

The palisade cells of the cotyledon which at first are united together in all directions very soon separate forming plates of tissue and the threads in the walls along which separation takes place are very quickly obliterated. A similar obliteration of threads is seen to occur in those walls of pericycle cells which are situated between the living cells and the young transfusion cells in process of lignification.

The living cells of the pericycle which are richly connected together by threads form the passage cells from the cortical tissues to the phloem and between these cells and the sieve tubes come the albuminous cells which possess thread groups occurring in localised thickenings of their walls. The threads which are long and usually curved stain in a peculiar manner and appear to have an important function with reference to the passage of material from the mesophyll to the phloem.

The phloem tissues of the seedling of *Pinus pinea* present a distinct type, the peculiarities of which are treated of at some length. The large cells of the outer portion are characterised by long oblique end walls full of threads whilst the thick walled cells of the inner part possess square end walls traversed by numerous long threads, resembling the sieve tubes of dicotyledons. As development proceeds, sieve tubes like those of the adult tissues are, however, quickly developed from the cambium. All the sieve tube threads show a characteristic median dot.

The root cap of the seedling root shows numerous threads connecting its cells together and also affording communication both with the free surface of the root as well as internally with the cells of the periblem. The function of the root cap as an organ for stimulus perception and as an absorbent organ is considered with reference to the abundance of the connecting threads.

In *Pinus sylvestris* the characters of the threads in the cortical tissues of the adult stem and root are similar to those of the seedling. Threads occur, however, in the radial and end walls of the cells, but in the cells just under the cork they are distributed in large numbers in the tangential walls, and this change in the main direction of the threads points to their value as a means of conducting food material to the developing cork

In the phloem there is a sharp contrast between the starch containing medullary ray cells and bast parenchyma on the one hand, and the sieve tubes on the other, and no threads can be found directly connecting the parenchymatous cells with the sieve tubes, but the albuminous cells of the ray possess numerous thread groups which communicate with both tissues The starch medullary ray cells in the phloem and xylem possess numerous threads in the tangential and basal walls especially in the former and are also united with the bast parenchyma and albuminous cells

The sieve tube threads which occur only in the radial walls always show a median dot

The existence of threads in the xylem is doubtful All living parenchymatous cells show them but it seems probable that they quickly disappear when the cells become lignified In the case of young bordered pits there is some evidence that the torus is traversed by connecting threads which are soon obliterated

The leaf of *Pinus sylvestris* shows a distribution of connecting threads similar to that noticed in the cotyledon The endodermis is seen to be an important layer connecting the tissues of the stele with those of the cortex by means of thread groups in the tangential walls In the pericycle there are both dead and living cells, but no threads persist in the walls connecting the dead with the living cells

The albuminous cell thread groups are very well developed, and their function and peculiar properties are discussed

In conclusion, the general distribution of the connecting threads throughout the tissues is considered

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"On the 'Blaze Currents' of the Frog's Eyeball" By A D Waller M D, F R S

Received December 6—Read December 6 1900

(Abstract)

The normal electrical response to light is positive The normal electrical response to every kind of stimulus is positive The normal response of the frog's eyeball is partly retinal, partly by other tissues The direction of response is reversed by pressure

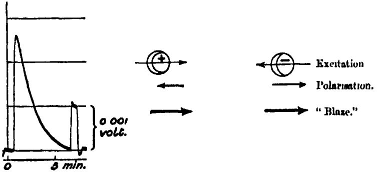
The normal "blaze currents" excited by single induction shocks, and by condenser discharges, are comparable with the normal discharges of an electrical organ Their maximum voltage is of the same order as that of the discharge of a single electrical disc (over

0·03 volt). Their magnitude and duration increase with increased strength of excitation.

Summation of stimuli, summation of effects, staircase increase, and fatigue decline are manifested by blaze currents. Stimulation of excessive strength abolishes them completely, but only temporarily.

The energy of a blaze effect may considerably exceed the energy of its exciting cause. The effects are observable for at least five days after excision of the eyeball; they appear to be diminished under prolonged illumination, and increased under prolonged darkness.

Fig. 1.



Positive response to a single induction shock sent through the eyeball in the positive (upward) direction.

The influence of raised temperature and increased pressure is studied, and under the influence of the latter four types of response are recorded.

Comparison is made between blaze currents and the responses of electrical organs as described by du Bois Reymond.

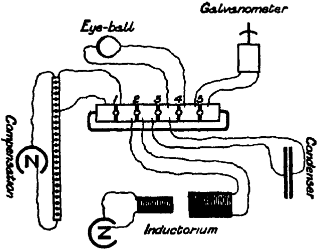
During and after maximal blaze the resistance diminishes; the diminution is not reciprocal.

If single electrical currents are passed through a normal eyeball and a galvanometer, in a "homodrome" and in a "heterodrome" direction (i.e., with and against the direction of normal discharge), the homodrome (positive) deflection is greater than the heterodrome (negative) deflection. This inequality is the result of positive blaze current, and is abolished by death or strong tetanisation. In the latter case the abolition is temporary.

The normal electrical response to light persists undiminished at a time when blaze currents have been abolished by tetanisation. On the other hand, blaze current may be present in an eyeball giving no response to light. The altered state of the eyeball in relation to light does not necessarily run parallel with its altered state in relation to electrical stimuli.

Direction of exciting current		Direction of organ response Living	Dead
[illegible]	Dorsum + ↑ Venter	+ ↑	− ↓
	Dorsum − ↓ Venter	+ ↑	+ ↑
Eyeball	Cornea + ↑ Fundus	+ ↑	− ↓
	Cornea − ↓ Fundus	+ ↑	+ ↑

Fig. 2



Plan of circuit

"On a Bacterial Disease of the Turnip (*Brassica napus*)." By M. C. POTTER, M.A., F.L.S., Professor of Botany in the University of Durham College of Science, Newcastle-upon-Tyne. Communicated by Sir M. FOSTER, Sec. R.S. Received November 15,—Read December 6, 1900.

In the autumn, when the activity of the turnip plant is mainly devoted to the storage of reserve material and the characteristic roots are increasing in size, it is not uncommon in this neighbourhood to find among the plants still growing in the fields some whose roots are quite rotten and with a highly offensive and peculiar smell.

The plants thus affected can be recognised by the drooping, yellowish leaves, the older leaves being the first to show any indications of disease. They gradually flag and droop to the ground, at the same time becoming yellow and shrivelled in appearance. The leaves next in age gradually exhibit the same signs of premature decay, and this proceeds until finally the young leaves at the growing point succumb. The time taken for the collapse of the leaves naturally varies with different individuals, but it is usually about two weeks from the time of the first infection.

The roots of these plants when examined present a very characteristic appearance. The decaying portion may be of a greyish-white or dark-brown colour, and is quite soft to the touch; the cell wall has lost its natural firmness and the cells their turgidity, and with the escape of the cell-sap the tissues have been reduced to a soft watery pulp. In the particular disease now treated, the portion attacked remains of a whitish colour, and I have therefore described it under the name "White Rot," as my investigations have shown that this form of rottenness is due to a specific organism producing this particular colour when attacking a root. The brown and other discolorations found in similarly diseased roots are probably due in part to this organism, together with others, but I have not succeeded in cultivating the "Brown Rot," and this awaits further investigation.

The disease can be readily communicated to sound roots, it being sufficient merely to make a slight incision and smear a small portion of the rotten mass upon the injured surface for decay to be immediately set up. In twenty-four hours the previously healthy cells around the inoculated surface show the characteristic changes of form and colour to a depth of about a quarter of an inch, indicating the progression of the decay. Keeping the plant under observation, without further injury, it is noted that the rind bordering on the wound gradually becomes soft and assumes a lighter colour; the discoloration gradually extends; the older leaves, too, droop and change colour;

by degrees the entire rosette of leaves perishes and the whole root becomes a soft, putrid mass, which eventually collapses, and after a shower of rain almost entirely disappears, exactly the same symptoms appearing as in the case of the plants found decaying in the fields. The most careful microscopic search has failed to detect any trace of hyphæ of the higher fungi in the decaying mass, but only a swarming mass of bacteria. The tissues are completely disorganised (see fig. 1), the cells separating from each other along the middle lamella, the cell walls are soft, swollen, and faintly striated, the protoplasm too has lost its natural colour and become slightly brown and contracted, so that it no longer remains closely in contact with the cell wall.

With a view to determine whether the bacteria are the cause of the rottenness, and if so to isolate the particular organism which produces it, a series of cultures was undertaken.

In the first instance a nutrient broth made from turnips was employed. Pieces of turnip finely chopped were steamed in a beaker until soft, sufficient tap-water being added to just cover them; when soft they were pressed through a cloth and the liquid filtered. To the clear light yellow filtrate thus obtained 5 per cent. of gelatine was added, and the mixture was then steamed, filtered, and drawn into test tubes, which previously had been plugged with cotton wool and exposed to a temperature of 140° C. for half an hour. These test tubes, containing about 10 c.c. each of the bouillon, were next steamed for half an hour on three consecutive days, and as a further test of complete sterilisation they were incubated at 20° C. for a few days. No colonies were found to develop. (Whenever mention is made of test tubes containing nutrient gelatine it must be understood that all have been prepared in this manner, and none have been employed which have not been submitted to these tests.) In some cases the broth was neutralised, in others it was allowed to retain the natural acidity of the cell sap; but subsequently Koch's bouillon, neutralised with sodium hydrate by the phenolphthalein test, was found to give the most satisfactory results, and hence was always used.

In separating the various organisms found in the rotten mass a sterile platinum wire was introduced into the turnip (the rotten part practically offering no resistance) and then immersed in a test-tube (A) containing about 10 c.c. of the liquid nutrient gelatine. From this a loop was taken in a similar manner into a second test-tube (B), and so on until a sufficient degree of attenuation was reached. The test tubes after being well shaken were turned out into petri capsules *a*, *b* . . . *g* respectively. These were placed in a cool incubator, and the colonies allowed to develop. In *a*, and often in *b*, the entire surface became covered with growing colonies too thickly crowded to be of any use for the purpose of isolation; but in the others the colonies were less numerous and sufficiently distinct to allow the organisms to be separated from each other The most conspicuous colonies were those which liquefied the gelatine among others producing no liquefaction *Micrococcus candicans* and a yeast were especially noted for their frequent occurrence but no trace of any of the higher fungi was found The colonies were next transplanted by means of a sterile platinum wire into test tubes containing about 10 c c of nutrient gelatine and after numerous trials I was satisfied that pure cultures were obtained

The various organisms as isolated were sown by means of a freshly heated platinum wire upon sterile but living blocks of turnip To prepare these blocks the turnips were first washed and then soaked in a 1 per cent solution of corrosive sublimate to destroy any organisms adhering to the outer surface the corrosive sublimate being afterwards thoroughly washed away by means of water sterilised by discontinuous boiling The rind was then removed by a sterile knife the turnips being cut into suitable blocks on a sterile plate and quickly inserted in the test tubes Treated in this way the blocks of turnip while quite sterile were composed of healthy living cells as was shown by three sets of control tubes In the first set the blocks prepared as above, were immersed in cooled liquid nutrient gelatine in the second similar blocks were immersed in sterile water in neither case were any colonies found to develop either when the blocks were partially or wholly submerged and after eight days no sign of decay had appeared In the third set the blocks were simply inserted in the tubes and kept in a damp atmosphere on microscopical examination cell division was observed to have taken place in the outer layers of uninjured cells and the cell tissues presented a normal and entirely healthy appearance

In the tubes containing the inoculated blocks many showed signs of advanced decay in about twelve hours and all those in which any rottenness appeared were carefully noted

After repeated experiment and a long series of cultures I succeeded in isolating a bacterium which liquefies gelatine and which when sown on the sterile blocks of living turnip produced the characteristic White Rot previously described

The isolation of the bacterium in this manner was further confirmed by picking out the colonies by means of Unna's harpoon Small colonies of about 15 μ growing in a petri capsule were selected and transplanted by the harpoon into petri capsules containing some sterile turnip bouillon A specially fine harpoon needle was obtained but the point was still larger than these very small colonies and it was only after some practice that they could be successfully transplanted The colonies selected were those growing quite apart which appeared to have arisen from a single bacterium to eliminate as far as possible any chance of the needle touching more than one Lest, however, even these small colonies might have grown from more than one bacterium, a single bacterium was selected and its development watched with the capsule fixed under the microscope until the colony was sufficiently large to transplant Cultivations were also made by the method of the hanging drop A drop of gelatine bouillon from a test tube containing a very few bacteria was placed upon a sterile coverslip and then inverted over a sterile growing cell and examined under the microscope If the bacteria were too numerous the preparation was discarded and trials made until a hanging drop was secured with only one or two bacteria The growing cell was now fixed under the microscope, so that a selected bacterium could be observed and the growth of the colony noted When sufficiently large the coverslip was quickly inverted and the colony removed by the fine Unna's harpoon to a petri capsule In this way pure cultures were obtained grown from a single bacterium which always gave rise to the characteristic “White Rot and left no doubt that this bacterium is the sole organism concerned in the disease

Pure cultures were also sown upon plants growing in the College garden with exactly the same result The decay commenced at the point of infection and soon spread through the sound roots eventually producing the same white putrefying mass of rottenness

The bacterium can live for many generations as a saprophyte without losing its virulence as a parasite A stock obtained from a white rotted ’ turnip growing in a field near Newcastle on September 10th, 1898, was isolated during that month and after passing through several cultivations in successive test tubes was finally put aside on April 29th 1899 On August 23rd two sound turnips were selected in the College garden, and while still growing the part of the roots above ground was washed with corrosive sublimate and afterwards with sterile water, a wound was then made with a sterile knife and a little of the culture from one of the test tubes left undisturbed since April 29th was introduced by a platinum wire The turnips were then covered over with a zinc cylinder, and upon examination five days after, on August 28th, the rot was found to have penetrated deeply into the tissues the larger half of the roots having become completely rotten with all the distinctive characteristics of the true White Rot

In order to ascertain the precise action of the bacterium, and to determine whether it produced any ferment capable of acting upon the cell wall in a manner similar to those of various parasitic fungi the method of precipitation by alcohol was adopted A litre flask was plugged, sterilised, and then filled about half full with sterile blocks of turnips, to which was added a small block upon which a pure culture of bacterium had been sown a little sterile water was then introduced, the flask closed as quickly as possible, and then well shaken to distribute the bacteria In twenty four hours many of the blocks showed the characteristic action of the bacterium, and in the course of three or four days nearly the whole contents had become rotten.

The next important step was to separate the bacteria from their products. The contents of the flask were turned out and pressed through a cloth into a glass cylinder to remove the coarser portions, the turbid liquid was then filtered and afterwards diluted with four to five times its bulk of alcohol. Almost immediately on addition of the alcohol a cloudy precipitate formed, and, at the end of twenty four hours, a copious flocculent precipitate was deposited. After filtration the precipitate was washed with absolute alcohol, dried, carefully collected and then digested with distilled water for about three hours. The solution was then passed through a Pasteur Chamberland filter fixed in a Maassen's bacteria filter. In this manner a clear pale straw coloured liquid was obtained free from bacteria. The liquid when drawn into sterile test tubes remained clear for any length of time but when exposed to the air it soon became turbid. A series of ten such sterile test-tubes was prepared five of which were held over a Bunsen burner, and the fluid allowed to boil, the other five were left without any exposure to heat. Thin sections cut from sterile blocks of turnip, by means of a razor steeped in boiling water, were taken off in sterile water and quickly introduced both into the boiled and unboiled fluids. The action of the unboiled fluid was very marked. Fig. 1 shows a

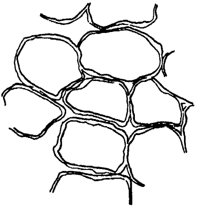


Fig. 1.—Group of cells from a section of turnip which has been exposed to the action of the cytase for twenty four hours. The cell walls are swollen and irregular in outline and the cells are separating along the middle lamella. (Zeiss, E oc. 2.)

section taken from one of these preparations after twenty four hours' exposure: the cell wall is swollen and striated, and so much softened that great difficulty was found in handling the section and removing it to the slide; it is well seen that the walls have quite lost their natural firmness and clear regularity of outline, being bulgy and distended in places; the dissolution of the cells is very apparent along the middle lamella, and the whole appearance of the section corresponds exactly with those taken from turnips found affected by the rot in the fields. The sections contained in the boiled fluid exhibited none of the appearances described above, and the cell-walls remained perfectly normal. It is thus evident that the bacterium secretes an enzyme which dissolves the middle lamella and causes the softening and swelling of the cell-wall Fig 2 represents a single cell from a section immersed in the filtered, unboiled liquid for sixteen hours. Fig. 3 shows one after an immersion of forty hours. The thickness of the walls was 2 μ and 5·3 μ respectively. (I should remark here that these sections were cut out of season from old turnips in which the walls would be more resistant, and this would account for the rela-

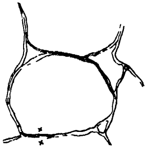


Fig 2.—Cell immersed for sixteen hours in an unboiled solution of the cytase Thickness of cell-wall, 2 μ at × × (*Zeiss*, E oc 2).

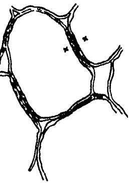


Fig. 3.—Section immersed for forty-two hours in unboiled solution of the cytase. Thickness of wall, 5·3 μ at × × (*Zeiss*, E oc 2)

tively slow development In sections from more succulent growing roots the walls have been found to swell from 2 μ to 7 μ in the course of twenty four hours) In fig 4 the cell is drawn from a section immersed for forty hours in the boiled liquid The cell wall is not perceptibly thickened or affected in any way

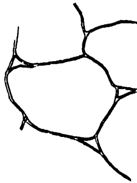


Fig 4—Section immersed in solution of extract of forty two hours whose power had been destroyed by boiling Cell walls quite normal (Zeiss E oc 2)

The activity of the enzyme in the decaying plant was also shown by passing the juice from the bruised pulp directly through a Pasteur Chamberland filter when its action on the cell wall was precisely that described in the case of the watery extract of the alcoholic precipitate

The bacterium also secretes the enzyme when growing in a beef solution Small flasks containing 100 c c of beef bouillon inoculated with a pure culture, became turbid in the course of twenty four to thirty six hours After an interval of eight days the liquid was filtered and diluted with five times its bulk of alcohol when a precipitate immediately began to appear After standing twelve hours the precipitate was collected by filtration dried, and then digested with 10 c c of distilled water After filtration through a Pasteur Chamberland filter experiments were repeated as above with sections of sterile turnip, and the same results were obtained, the liquid was found to possess the property of dissolving the middle lamella, and causing the softening and swelling of the cell wall All action of the ferment was destroyed by boiling

To avoid the tedious process of the filtration through a Pasteur Chamberland filter, and the necessary sterilisation of the apparatus, various attempts were made to render the solutions aseptic by the use of such re agents as chloroform, thymol, formalin, &c But this process had to be abandoned, as in all these cases living bacteria were found after twenty four hours, and no reliance could be placed upon it

In the early stages of the investigations, filtration—except when the elimination of bacteria was desired—was effected through ordinary filter paper. The quantity of the precipitate being small the portion of the paper upon which it was deposited was cut out and digested with water. But in order to avoid any possible action of the enzyme upon the paper kieselguhr was subsequently invariably employed, a few pieces of glass at the base of the funnel covered with a little asbestos serving to prevent the kieselguhr from passing through, the necessary pressure to ensure filtration being derived from an air pump.

The filtered extract from the rotten turnip also contains a diastasic ferment. Two test tubes each containing 5 c.c. of the dissolved ferment were diluted with 5 c.c. of a 1 per cent. starch emulsion one of the test tubes having previously been boiled. After twenty four hours the test tube with the unboiled ferment showed no starch reaction on the addition of iodine but the boiled tube at once gave the characteristic blue.

Similar diastasic enzymes are excreted by several other bacteria (Lafar.)

It will be convenient here to say that adopting Migula's classification I have ventured to name the bacterium I am describing *Pseudomonas destructans* though the description will be given later.

It has been established that *P. destructans*, both when living in a nutrient solution and on a living turnip excretes an enzyme which has the power of dissolving the middle lamella and of causing the softening and swelling of the cell wall.

As a further result of the bacterial action as already described, the protoplasm of the cells is found to have contracted become brown and separated from the cell wall showing evidence of the action of a toxin secreted by the bacterium. The same effect was produced in living turnip cells when treated with the boiled pressed juice of a turnip which had become rotten through the influence of a pure culture of *P. destructans*. The pressed juice was filtered and about 10 c.c. drawn into test tubes, which were then plugged and sterilised by discontinuous boiling. Sections cut by a razor sterilised by boiling, from blocks of sterile, living turnip (p. 444) were quickly transferred to the boiled juice after it had cooled, at the same time similar sections were immersed in test-tubes containing the same quantity of sterile water. After twelve hours a very marked contrast was observable between these sections. In those immersed in the sterile water the cells presented the normal appearance, with the protoplasm pressed close to the cell wall while in those in the boiled pressed juice the protoplasm was dead, had assumed a brown tint, and contracted away from the cell wall. A toxin, therefore, which is not destroyed by boiling is secreted by *P. destructans*.

In his paper "Ueber einige Sclerotinien und Sclerotien Krank heiten,' de Bary has shown that oxalic acid is secreted by the hyphæ of *Peziza sclerotiorum* when living as a parasite, and that this acid acts as a toxin in killing and plasmolysing the protoplasm. Wehmer has found that *Aspergillus niger* and *Penicillium glaucum* also form oxalic acid when growing in a sugar-containing solution. Oxalic acid, being an unavoidable product in the metabolism of the higher plants, and also in some fungi, it seemed reasonable to suppose that it might be found as a similar product in the life of bacteria. With this idea I tested the juice expressed from a rotten turnip, and found, on addition of calcium chloride, a precipitate which proved to be calcium oxalate. Cultures were then undertaken to test for the presence of oxalic acid as a product from *P. destructans*. A broth, made by steaming small pieces of actively growing turnips until soft, was neutralised by an excess of calcium carbonate and filtered; it was then allowed to stand overnight, when a further deposition of calcium took place; it was then again filtered, clarified with the white of an egg, steamed, filtered, and drawn into four flasks, each containing 150 c.c., which were sterilised. A solution was thus obtained free from any oxalic acid which might have been present in the tissues of the turnip. Two of the flasks were inoculated with *P. destructans* on August 28th. In twenty-four hours they became turbid, and after four days were tested and found to contain oxalic acid, while the control flasks showed no evidence of this acid, and remained perfectly clear.

P. destructans also sets up an oxalic fermentation in Pasteur's solution. A litre of Pasteur's solution with cane sugar was made up and divided into four flasks, each of which was carefully sterilised and one sown with *P. destructans*. After twenty-four hours the liquid in the inoculated flask, which was previously perfectly clear, became cloudy, and after a week quite opaque; 10 c.c. of this, when treated with a solution of calcium chloride, in the presence of acetic acid, at once showed a precipitate of calcium oxalate, which increased on being warmed. Another 10 c.c. of the original solution, which had been kept sterile during the same period, remained quite clear on treatment with the same reagents. *P. destructans* thus sets up an oxalic fermentation in a sugar-containing liquid. It has also been found that carbon di-oxide is given off during the process.

When treated with alcohol, the Pasteur solution, in which *P. destructans* had been growing for eight days, yielded a white flocculent precipitate which contained the cytase. The oxalic acid, however, remained in solution, and was deposited as the calcium salt on addition of calcium chloride. This calcium precipitate, when mixed with manganese di-oxide and treated with sulphuric acid, yielded carbonic acid, which furnishes a further confirmatory test of the presence of oxalic acid. The precipitation by alcohol affords a ready method of separating the toxin (oxalic acid) from the cytase, and this explains why the sections treated with the watery extract of the alcoholic precipitate exhibited no marked plasmolysis.

That the oxalic acid formed by *P. destructans* in Pasteur's solution acts as a powerful toxin was very clearly shown. Six plugged and sterile test-tubes were prepared, and about 10 c.c. of the fermenting Pasteur solution was poured into each. To three of these (series I) sufficient calcium carbonate was added to neutralise the oxalic acid. Both series of tubes were then sterilised by discontinuous boiling, during which process the cytase would be destroyed, and into both when cool freshly cut and sterile sections of turnip were placed, prepared as described on page 446, and the solutions allowed to act till next morning. The sections in the second series of test-tubes showed a marked contraction of the protoplasm, and it looked brown and dead, and showed no tendency to return to its normal condition when immersed in pure water. In the first, which were treated with calcium carbonate, the protoplasm was quite normal, and exactly resembled a section which had been immersed in sterile water for the same period.

A third set of test-tubes were filled with about 10 c.c. of the solution; these were not boiled, and received no calcium carbonate; the sections introduced showed complete dissociation of the cells, the cell walls greatly swollen and the protoplasm very strongly contracted. This experiment with the Pasteur solution demonstrated the production of the same cytase, and strikingly illustrated its effect upon the plant cell, as well as the toxic action of the oxalic acid, even more so than was the case with the same experiment with turnip juice.

In considering the effect of the oxalic acid upon the cells, it is important to note that calcium pectate, a salt which is decomposed by oxalic acid with the production of calcium oxalate, enters largely into the composition of the middle lamella. Wehmer has shown that in the cultivation of *Aspergillus niger* and *Penicillium glaucum* oxalic acid is formed in saccharine solutions, that the oxalic acid produced acts as a toxin to these fungi, and gradually diminishes their vigour, and that when a certain strength has accumulated no further development is possible; growth, however, is resumed when the oxalic acid is neutralised by a calcium salt. The reaction between the oxalic acid produced by *P. destructans* and the calcium pectate of the middle lamella is precisely analogous; the oxalic acid would be neutralised, and the pectate replaced by the oxalate, and the continued growth of the bacteria would thus be rendered possible. The oxalic acid* then both acts as a toxin in killing the cells and may also play some part in the destruction of the middle lamella and the separation of the cells

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* Since the above account of the formation of oxalic acid by *P. destructans* was written, Zopf has published a note also describing the formation of oxalic acid by *B. xylinum*, 'Oxalsäurebildung durch Bacterien.' 'Berichte d. D. Bot. Gesell.' Feb. 1900.

Fig 5 shows a cell swarming with *P destructans* the bacteria are seen occupying the inter cellular spaces and lying in the track of the middle lamella

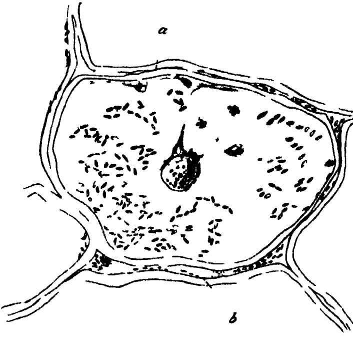

Fig 5—A cell from turnip inoculated with a pure culture of *P destructans* The bacteria are seen in the cell cavity and also along the track of the middle lamella and in the intercellular spaces The cell wall is much swollen at *a* it is just beginning to separate along the middle lamella and at *b* the dissociation is more strongly marked The nucleus and portions of the protoplasm still remain (Drawn with Abbe camera lucid Zeiss E oc 4)

In the case of several parasite fungi, the hyphæ also burrow in the thickness of the cell wall, and the same phenomenon is now shown to be true of one parasitic schizomycete, and possibly this is owing to the necessity for the neutralisation of the oxalic acid as a condition of existence

Enzymes similar in nature to that described for *P destructans* have been demonstrated by Marshall Ward for *Botrytis* and by de Bary for *Sclerotinia*

The action of this bacterium upon living plant tissues is precisely similar to that of certain of the parasitic fungi, in both cases the invading organism produces oxalic acid, which acts as a toxin to the protoplasm and, decomposing the calcium pectate, furthers the dissolution of the cells; and also there is the secretion of a cytase, which has a destructive action upon the cell-wall and intercellular substance. The question of the parasitism of the bacteria thus stands in these respects on the same platform as that of the fungi, and a complete homology is established between them

At first I experienced considerable difficulty in staining the flagella. Loeffler's method was first tried, but with no positive results; it enabled one, however, to notice two deeply stained portions at either end of the rod. Van Ermengen's method also failed in spite of the strictest attention to technique, but by gradually increasing the strength of the silver nitrate, and finally using a 2 per cent solution, the desired result was obtained, and the bacillus was then found to possess *one polar flagellum* (fig. 6) It should be mentioned that the practice of passing the cover-slip through a flame was discarded in favour of drying the cover slip at 60 C in a water bath, the latter method being more certain and giving better results.

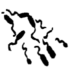

Fig 6.—*Pseudomonas destructans* with single polar flagellum. (Swift's 1/12th apochromatic and compensating ocular 12.)

Pseudomonas is aerobic A stab culture rapidly develops along the track of the wire, forming a funnel, the edge of the funnel reaches the sides of the test-tube in about forty-eight hours, and gradually sinks as the gelatine becomes liquid. The gelatine, however, is never wholly liquefied, the liquefaction extending down the sides of the tube only to a depth of about one and a half centimetres. If a layer of gelatine is immediately poured above the stab and the test-tube placed in the incubator, the track of the wire is clearly marked out as before, but the colonies soon cease to develop, and all growth ceases after three days. The tube may be kept for many weeks in this condition.

Again, so far as my experiments show, the action of *Pseudomonas* upon turnips and potatoes only takes place in the presence of oxygen. The following are typical examples of experiments frequently repeated and always with the same results:—A flask holding about 250 c.c., with a tightly fitting indiarubber cork perforated to admit two glass tubes bent at right angles, was sterilised in the following manner. The tubes were plugged at each end with cotton wool, and the plugs pushed well into the tubes, the flask being also plugged with cotton wool, and, together with the glass tubes, sterilised by dry heat. Meanwhile the indiarubber cork was boiled for half an hour in a 10 per cent solution of corrosive sublimate The flask having cooled was then about half filled with sterile blocks of living turnip, prepared as described above and inoculated with a pure culture of *P destructans* The indiarubber cork after being washed in sterile water was quickly inserted into the flask the glass tube being pushed through the perforations and the junctions sealed with melted wax The longer tube (A) reached to the bottom of the flask the shorter (B) only slightly protruded downwards through the cork The action of the bacterium could be detected in the course of twelve hours, the blocks changing colour and showing signs of disintegration at the edges During fermentation a considerable quantity of gas was given off which could be collected from B over a pneumatic trough, the fluid which soon accumulated at the bottom of the flask rising in A, supplying the requisite pressure When the longer tube A was left open and a sufficient supply of oxygen could diffuse into the flask carbonic acid was continually given off and in the course of about a week the contents became entirely rotten and reduced to a watery mess When, however in a precisely similar flask used as a control the longer tube after a short interval was closed and the shorter connected with a tube for collecting any gas given off thus cutting off the supply of oxygen the evolution of CO soon ceased and as far as could be observed the action of *P destructans* ceased also

To carry this point a step further and to ascertain more definitely whether the action of *P destructans* could take place in the absence of oxygen another series of flasks was fitted up with the two tubes as already described the same precautions as to sterilisation being adopted and the prepared blocks of turnip introduced and inoculated as before The shorter tube was now connected with a second flask containing an alkaline solution of pyrogallic acid, and the other with a bent tube containing mercury to act as a manometer, and prevent any access of oxygen from the air The first result noticed was an expansion of the air in the flasks, the mercury rising in the distal limb The mercury continued to rise, bubbles of carbon dioxide eventually escaping round the bend This action, however, ceased in the course of two days, the available supply of oxygen in the flasks and intercellular spaces being exhausted After a long interval (four months—June 6 to October 5) the flasks were disconnected, and the turnip blocks examined They still retained their original shape, and were only rotten superficially, the pieces had somewhat lost their rigidity, but offered considerable resistance when stretched Microscopic examination showed all the cells to be dead, but it was only one or two layers of superficial cells which showed any evidence of bacterial action The cell walls on the outside of the block were swollen and striated, and could be readily separated along the middle lamella, the cell walls in the interior of the tissue however, presented the normal appearance neither swollen nor readily separating.

Control experiments were set up in which after four days, the manometer and pyrogallic flask were disconnected, and the air allowed to diffuse into the flasks upon subsequent examination the blocks in these were found to have become completely rotten. We may thus infer that the action of *P. destructans* only proceeds so far as a supply of oxygen is available.

Potatoes as well as turnips were employed in these experiments and the results in each case were the same except that with the potato when the flask was connected with the pyrogallic flask and manometer, immediately after the inoculation of the blocks no bubbles of CO_2 were observed to escape round the bend and there was no indication of the rot.

Characters of Pseudomonas Destructans

Habitat. On growing turnips producing a "White Rot" in the living tissues.

Morphology. Short motile rods $3\mu \times 8\mu$ with a single polar flagellum.

Cultures can only be made in the presence of oxygen.

Gelatine Plate Cultures. Forms circular colonies of whitish grey liquefying gelatine.

Stab Culture. Grows rapidly along the track of the wire, forming a funnel-shaped tube of liquid gelatine with a white, cloudy deposit in the liquid portion.

Agar. White glazy growth.

Turnips. Grows rapidly as a parasite.

Potato and Carrot. Same effect as on the turnip.

Beetroot. No growth as a parasite.

Broth.—Koch's bouillon and turnip becomes cloudy and opaque.

Ferments. A cytase causing the swelling and softening of the cell wall and dissolution of the middle lamella.

A diastase. A peptonising ferment, producing liquefaction of gelatine.

Toxin.—Oxalic acid formed as a product of metabolism in turnip-juice and in Pasteur's solution containing cane sugar.

Stains.—Readily stained with the ordinary aniline dyes, but not with Gram's method.

Reaction.—Residual product always acid.

Copious evolution of carbonic acid during the fermentation.

Among various bacteria at present noted as causing plant diseases, that described by Kramer as attacking the potato (Nassfaule) approaches most nearly to the one which is the subject of this paper. Kramer's bacillus agrees in liquefying gelatine very rapidly, and it destroys the middle lamella, and finally the cell wall. The size of the bacterium as given by Kramer is from 2·5 μ long and ·7–·8 μ broad, very nearly the same dimensions as those of the turnip bacterium. Kramer however has not named his bacillus, and he makes no mention of the flagellum. He describes two stages in the decay of the potatoes. First an acid stage during which butyric acid and carbonic acid are given off; in this stage the sugars, then the intercellular substance, and finally the cell walls are destroyed, the starch is not attacked. Subsequently the proteids are broken up with the formation of ammonia, methylamine, trimethylamine and other products; in this stage the acids are neutralised. In the action of *P. destructans* upon turnips and potatoes carbonic acid is given off and the reaction of the pulp is always acid. On referring to a chemical friend he could not definitely state that butyric acid, methylamine, and trimethylamine are also produced; he was of opinion that they were present, but that the decomposition is of a more complicated nature. *P. destructans* differs from Kramer's bacillus in secreting a diastase and always yielding an acid product; further, *P. destructans* liquefies the gelatine in circular areas, the leaf-like formation described by Kramer never having been observed; nor have I ever found the apparently unjointed threads as much as 16 μ long upon nutrient plates. Pammel and Smith have also described a *Pseudomonas* (*P. campestris*) which causes a brown rot in the root and leaves of various cruciferous plants, evidently quite a distinct form.

The action of the bacteria upon the cell wall of the higher plants has been studied by several observers. Van Tieghem, probably working with mixed cultures, has ascribed the destruction of cellulose to *Bacillus amylobacter*. Van Senus has isolated an enzyme and demonstrated its solvent power upon cellulose from two bacteria, one anaerobic, living symbiotically. Winogradsky and Fribes have isolated an anaerobic bacterium which dissolves the middle lamella in the process of flax-retting, and sets free the bast fibres, without, however, having any action upon the cellulose. Arthur ascribes the action of bacteria in the bacteriosis of carnations to an enzyme, but without isolating it.

Till quite recently I was unaware that any one had isolated from the bacteria an enzyme capable of attacking the middle lamella of living cells, and thus causing a plant disease. Laurent's valuable paper, "Recherches Expérimentales sur les Maladies des Plantes," I only obtained in August of this year. It was published in December, 1898, simultaneously with a preliminary paper I read at the University of Durham Philosophical Society; but previously, as early as January, 1898, I made a brief report to the Royal Society embodying the results of my work, viz., the isolation of the specific bacterium causing the

White Rot of turnips, and the isolation of an enzyme which dissolved the middle lamella and caused softening and swelling of the cell wall. The pressure of teaching has prevented my publishing the complete paper sooner.

Laurent in his investigations upon the potato and the causes of its greater or less resistance to bacterial disease also established the existence of a cytase which dissolved the middle lamella, rapidly softened the cell tissues and caused the disaggregation of the cells.

The organism which was the chief subject of Laurent's researches, *B. coli communis*, is very rarely capable of living as a parasite upon potato-tubers and other plants. He states that it was necessary for the tubers to be deprived of resistance by means of exceptional cultures to enable the bacillus to develop upon the potato. From that point its virulence was increased by successive cultivations upon tubers of slight resistance, until varieties at first highly resistant ended by becoming invaded by the parasite. The virulence disappeared as soon as the microbe ceased to be cultivated on a living tuber; cultures in nutritive solutions sufficed to suppress the aptitude of the parasite, and henceforward it could only be restored after special preparation in alkaline solutions.

Ps. destructans, on the contrary, flourished on nutritive media, and even after many cultivations could readily be inoculated from these on to pieces of living turnip, producing all the effects of the rot in about twelve hours; cultures both on nutritive media and on the turnip also rapidly invaded the tissue of the potato. Whether therefore it has any existence in a saprophytic form or not, it has evidently become strongly established as a parasite attacking the turnip, and probably is not confined to the turnip alone.

Wehmer has recently attempted to show that bacteria are not parasitic in the case of the wet rot (Nassfäule) of the potato, and that their action is only secondary. He maintains that bacteria only attack dead or unhealthy tissue; that the warmth and moisture of the damp chambers impair the health of the cells, and infection is only possible under conditions which renders the tissues morbid. The wet rot, Wehmer says, begins with a maceration of the tissues; between the separating dead cells numerous small bubbles are to be seen and masses of a small rod-like schizomycete. The initial stage is one of pectin fermentation, succeeded by cellulose fermentation. With these processes are associated two special forms of bacteria. Wehmer's description of the rotting tissues agrees with my own, but he makes no mention of the enzyme nor of cultures of the bacteria. His conclusions that bacteria are not parasitic cannot be accepted in view of the isolation of the special enzyme by Laurent and myself, and of my experiments proving the infection of sound, healthy turnips when growing under perfectly natural conditions.

From numerous observations in the fields, I have come to the conclusion that *P. destructans* is always introduced at a wounded surface. Except in cases in which the decay has proceeded to a large extent, the point from which the decay spreads is always indicated by a wound in the epidermis and subjacent tissues. This observation is supported by the failure to infect sound roots except by first making a small incision, and from numerous trials it would appear that *P. destructans* is powerless to set up decay unless placed in contact with the parenchyma-cells of the cortex. Wounds caused by various snails, slugs, and larvæ, by which the bacterium could gain an entrance, are frequently to be seen on the roots, and I have no doubt the bacteria gain an easy entrance by this means. That slugs can and do carry the various disease producing organisms has been shown by Smith in the case of the cabbage brown rot by *Agriolimax agrestis* and the larvæ of *Plusia brassicæ*, and of the tomato brown rot by the larvæ of the Colorado beetle. G. Wagner's experiments also conclusively prove that the spores of various parasitic fungi are very commonly distributed by snails.

Bacterial disease of turnips is much more common than is generally recognised, and the one now described is often very destructive to the crops, not only in the field but in store during the winter. On examining numerous specimens sent me for investigation, I speedily found that what is generally known as "finger and toe" or "grub," is by no means confined to *Plasmodiophora brassicæ*, but that many other organisms, either singly or in combination, play a very important part in the destruction of living turnips and swedes. Finger and toe is everywhere so prevalent that in considering the nature of turnip attack it is often too hastily assumed that *Pl. brassicæ* is the sole cause of the disease, and that the other effects are merely secondary. In addition to bacteria and *Pl. brassicæ*, I have found the turnip and swede crops to be attacked by *Fusarium* and also by *Botrytis*, and it is probable these do not exhaust the list of vegetable parasites for this crop, but further research is necessary before it is possible to separate the various organisms and assign to each its *rôle*.

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"The Micro-organism of Distemper in the Dog, and the Production of a Distemper Vaccine."

By S. Monckton Copeman, M.A., M.D., F.R.C.P. Communicated by Sir M. Foster, Sec R.S.

Received November 14,—Read December 6, 1900

(From the Brown Institution)

Distemper is so fatal a disease of dogs, more particularly of such as are highly bred, that a method of preventing invasion by the disease has always been a desideratum.

As the result of investigations into the bacteriology of this disease, carried out in continuance of those commenced in my laboratory at St. Thomas's Hospital about ten years ago by the late Everett Millais, I find that the specific micro-organism concerned is a small cocco-bacillus, which stains with the ordinary aniline dyes, but is decolorised by the method of Gram. It grows readily on the surface of agar at body temperature; the individual colonies when isolated by the method of plate-culture having a greyish, glistening, semi-translucent appearance by reflected light, and a light-brownish tint by transmitted light. The general form is circular, but occasionally, and specially in primary growths, the edge is somewhat irregular. The microbe also grows well in beef-broth, causing at first a general turbidity. Later on, a deposit falls to the bottom of the tube, and the supernatant liquid becomes somewhat clearer. In cover-glass preparations from broth cultures the bacilli are not unfrequently found united together to form chains, sometimes of considerable length. The bacillus is capable of growing, though comparatively slowly, on solidified blood serum and also in milk which does not become coagulated. On potato it develops with difficulty, but now and again after some days incubation a moist-looking streak of a pale buff colour may be observed. If gelatine be inoculated growth occurs slowly at the temperature of the room, and, after a time the medium tends to become liquefied.

Growth on agar can be carried on week after week for a great number of generations but after a dozen removes or so its morphological and biological characteristics are found to have become somewhat altered. An account of these variations and of the pathological histology of the disease I propose to publish subsequently.

In similar fashion the pathogenetic properties of the micro-organism appear to become gradually weakened but by repeated intra-peritoneal inoculations in the guinea pig its virulence may be regained.

The injection beneath the skin of the abdomen in a dog weighing 7 kilos of 1 c.c. of a broth culture seven days old derived in turn from an agar sub-culture induced an attack of distemper, which terminated fatally in about a week from the time of inoculation. In a large number of other dogs experimented on by Millais or myself a generally non-fatal attack has followed on inoculation of the nasal mucous membrane.

Specially characteristic of the disease intentionally produced is the fact that the animal exhibits during the attack a marked and progressive loss of weight. Of other symptoms of the malady so well known to all dog breeders those which are usually most marked are the result of more or less acute inflammation of the various mucous surfaces.

On post mortem examination I have generally found the whole respiratory tract to be specially affected, the lungs sometimes showing pneumonic consolidation throughout almost their entire extent. The trachea is apt to be congested and to contain a quantity of mucus, while the eyes and nose are blocked with a purulent or muco-purulent discharge. By making agar plate cultivations from the exudation from the lungs, from the tracheal mucus, or from the nasal secretion, the specific bacillus may be isolated—from the first two situations, often in almost pure culture.

Examining animals which have died from distemper, whether resulting from experimental inoculation or contracted in the ordinary fashion, I have never succeeded in obtaining cultures either from the blood obtained from the heart with aseptic precautions, or from the liver, the gall bladder, the kidney, or the spleen. Pressure of other work since joining the Medical Staff of the Local Government Board, has prevented my having the opportunity of examining even inoculated animals at intermediate stages of the disease in severe forms, or, doubtless, it might have been found possible to isolate the bacillus from one or other of these situations. In one instance, in which the blood-vessels of the brain were found to be much congested, inoculation of a tube of sloping agar with a large platinum loopful of cerebro-spinal fluid, well spread over the surface of the agar, resulted in the appearance of half a dozen isolated colonies of a pure culture of the distemper bacillus.

By heating a broth culture of the bacillus at 60° C. for half an hour, and subsequently adding a small quantity of carbolic acid as a preservative, a vaccine is obtained, which acts in similar fashion to those devised by Haffkine and Wright for use in the prevention of plague and enteric fever respectively. The vaccine may be standardised after the manner originally suggested by Wright in connection with his work on enteric fever

The dose must obviously vary according to the size of the dog, but, as a guide, it may be mentioned that I have found, in three instances, that the injection of 2 c.c of the sterilised culture of the bacillus is apparently sufficient to protect fox-terrier puppies weighing about 1½ kilos. against attack by distemper, while an unprotected puppy in the same batch contracted the disease on introduction of an affected dog. I find also that guinea-pigs can be protected in this way against the effects of a dose of living culture, which would ordinarily prove fatal in about forty-eight hours. As regards the exact length of time, however, during which such protective effect may last, no definite statement can as yet be made, but a series of tests on a large scale are in process of being carried out by dog-breeders in this country, in Germany, and in America

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"On the Tempering of Iron hardened by Overstrain."[*] By JAMES MUIR, B.Sc., B.A., Trinity College, Cambridge, 1851 Exhibition Research Scholar, Glasgow University. Communicated by Professor EWING, F.R.S. Received July 11,—Read December 6, 1900

(Abstract.)

It is well known that iron hardened by overstrain, for example, by permanent stretching, may have its original properties restored again by annealing, that is, by heating it above a definite high temperature and allowing it to cool slowly. Experiments described in the paper, of which this is an abstract, show, however, that if iron hardened by overstrain be raised to any temperature above 300° C., it may be partially softened in a manner analogous to the ordinary tempering or

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[*] The work described in this paper is a continuation of that already described in a paper by the present author "On the Recovery of Iron from Overstrain," 'Phil. Trans.,' A, vol. 193, 1899.

'letting down' of steel which has been hardened by quenching from a red heat This tempering from a condition of hardness induced by overstrain, unlike ordinary tempering, is applicable not only to steel, but also to wrought iron and possibly to other materials which can be hardened by overstrain and softened by annealing

The experiments described in the paper were all carried out on rods of iron and steel about [illegible]ths of an inch in diameter and 11 inches long, the elastic condition of the material being in all cases determined by means of tension tests in which the hardness of the material was indicated by the position of the yield point The straining was performed by means of the 50-ton testing machine of the Cambridge Engineering Laboratory and the small strains of extension were measured by an extensometer of Professor Ewing's design, which gave the extension on a 4 inch length of the specimen to the 1/100,000th of an inch

For the purpose of tempering and annealing, a gas furnace was employed 2 feet in length, the specimens being protected from direct contact with the flame by inclosing them in a thick porcelain tube The temperature inside this tube was determined by means of a Callendar's direct reading platinum resistance pyrometer

The method of examining the materials employed is illustrated by the following two diagrams, in which the material examined is a ½ inch rod of semi mild steel (0 35 per cent C 1 per cent Mn) * Curve No 1 of the first diagram shows that this steel when in the condition as supplied by the makers gave a well defined yield point at about 38 tons per square inch, the material yielding at that stress by 0 13 of an inch on a 4 inch length

Curve No 2 illustrates the semi plastic state of the material, produced by just passing this primary yield point The specimen was laid aside for 1¾ days, then once more tested and Curve No 3 shows the progress made during this interval of rest towards recovery of elasticity Curve No 4 shows the condition of the overstrained material after it had been resting for two weeks To insure perfect recovery of elasticity, the specimen was heated to 200° C, but a few minutes at the temperature of boiling water would have been nearly as effective in restoring the elasticity lost by overstrain †

After cooling, the specimen was tested by reloading and carefully increasing the load above its previous maximum amount till a well defined yield point was obtained at 49 tons per square inch, as shown by Curve No 5, the yield point having thus been raised by the large step of 11 tons per square inch The yielding which occurred at this

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* Details of the special method adopted in plotting these diagrams will be found in the author's previous paper "On the Recovery of Iron from Overstrain" 'Phil Trans,' A, vol. 193 1899, p 12.

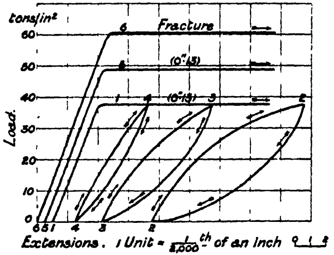
† *Ibid*, p 22

yield-point was the same as obtained in the first test of the specimen, namely, 0·13 of an inch on 4 inches.

The material after this second overstrain was once more in a semi-plastic state. A curve obtained immediately after the overstrain would have been similar to Curve No. 2, but the loading could have been continued up to 49 tons per square inch. Had the loading been continued beyond this amount while the material was in the semi-plastic state, large yielding would have taken place, and fracture would have occurred at probably a very slightly increased load. Recovery of elasticity was, however, effected as before, by heating the specimen to about 200° C., and allowing it to cool. It was known as the result of earlier experiments* that the yield-point of the material would be raised by this process through a second step of 11 tons, so that the specimen should not yield until a stress of 60 tons had been applied.

DIAGRAM 1.

(Steel as supplied.)



Curve 1.	Primary test.	Curve 4.	2 weeks after 1.
„ 2	Shortly after 1.	„ 5	After heating to 200 C.
„ 3.	1½ days „ 1.	„ 6.	„ „ „

Curve No. 6 of Diagram 1 shows that the specimen bore the stress of 60 tons, but that with 60½ tons per square inch, a yield-point and fracture occurred.

Diagram No. 2 shows that annealing altered in an interesting manner the elastic properties of the steel whose virgin properties are illustrated by Diagram 1. The primary yield-point was considerably lowered by annealing, and the step by which the yield-point was raised in consequence of overstrain and recovery from overstrain was considerably reduced.

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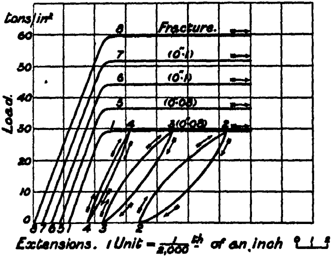
* *Ibid.*, p. 34.

The material in the condition as supplied yielded (as is illustrated by Diagram 1) at 38 tons per square inch, and after the yield-point had been raised by *two* steps of 11 tons, fracture occurred at 60½ tons per square inch. The same steel, after annealing at 750° C., is shown

Diagram 2.

(Steel annealed at 750° C.)



Curve 1. Primary test.
" 2. Shortly after 1.
" 3. 1¾ days " 1.
" 4. 2 weeks " 1.

Curve 5. After heating to 300° C.
" 6. " " "
" 7. " " "
" 8. " " "

by Diagram 2 to have yielded at 29 tons per square inch, and finally to have fractured at 59½ tons per square inch, after the yield-point had been raised *four* times by a step of about 7½ tons per square inch. The ½-inch steel rod when in the condition as supplied by the makers was thus shown to be in a state of hardness possessing certain distinctive properties.

It was found that the steel in the condition as supplied could be tempered or partially annealed by heating to various temperatures lower than the ordinary annealing temperature of about 750° C.

The following table illustrates this tempering from the condition as supplied, the material being a rod of steel very similar to that referred to above. The "steps" tabulated in the last column are the amounts by which the yield-points were raised in consequence of overstrain and recovery from overstrain:

Condition of the material.	Yield-point	Extension at yield-point.	"Step"
As supplied	36½ tons/in.^{2}	0″·16 on 4 inches	11 tons/in.^{2}
Annealed at 600° C.	36 ,,	—	—
,, ,, 650° ,, ...	33½ ,,	0″·16 ,, ,,	9½ ,,
,, ,, 700° ,, ...	31 ,,	0″·15 ,, ,,	9 ,,
,, ,, 730 ,, ...	28½ ,,	0″·09 ,, ,,	8 ,,
,, ,, 780 ,, ...	24 ,,	0″·07 ,, ,,	7 ,,

In order to show the tempering of steel hardened by tensile overstrain, a specimen of annealed steel was overstrained in a manner analogous to that illustrated by Curves 1, 5, and 6 of Diagram 2. The material, after recovery from overstrain, had thus been brought into a condition of hardness, which enabled the specimen to be loaded to 50 tons per square inch without a yield-point being reached. The specimen was then subjected to a series of tests after being heated successively to various temperatures, the result being to show that 310° C. produced no softening of the material, 360° C. lowered the yield-point to 47 tons; 500, 600°, and 700 C. lowered the yield-point to about 40, 35, and 30 tons per square inch respectively.

It was further shown that the same temperature brought the yield-point to approximately the same stress, no matter what might be the original hardness of the specimen under test, and that the harder the material was made by tensile overstrain—that is, the higher the yield-point was raised by permanent stretching—the lower was the temperature which could be shown to produce a slight tempering effect. Thus in the above instance had the material been made harder (by further overstraining) than was shown by the elastic range of from zero to 50 tons per square inch, then possibly the temperature of 310° C. would have produced a slight softening of the hardened material; a temperature of about 300° C. was, however, found to be the minimum temperature which had a tempering effect on the hardest condition of steel tested.

The tempering effects which have been ascribed above solely to temperature, were found to be influenced to some extent by time. Thus it was found that by baking a hardened specimen for several hours at any temperature a greater effect was produced than by simply raising the specimen for a few minutes to that temperature. The effect of time was, however, small compared with that produced by increase of temperature.

All the results which are described above for steel were also obtained with Lowmoor iron. The hardening by overstrain and the tempering of soft Lowmoor iron only differed in detail from the analogous hardening and tempering of steel

The iron and steel employed in this research were also examined, when in various conditions of hardness, by means of the microscope, and micro-photographs are reproduced in the paper. The ordinary methods of relief polishing and of etching by dilute nitric acid were employed, and a new method of staining steel, by rubbing with ordinary moistened cocoa, was made use of and is described in the paper.

December 13, 1900

Sir WILLIAM HUGGINS, K.C.B., D.C.L., President, in the Chair.

A List of the Presents received was laid on the table, and thanks ordered for them.

In pursuance of notice sent to the Fellows, an election was held to fill the vacancy upon the Council caused by the retirement of Sir John Wolfe Barry.

The statutes relating to the election of the Council, and the statute relating to the election of a Member of Council upon the occurrence of a vacancy, were read, and Professor Dewar and Mr. Godman having been, with the consent of the Society, nominated scrutators, the votes of the Fellows present were taken and Mr. Joseph Wilson Swan was declared duly elected.

The President made the following statement concerning the International Catalogue of Scientific Literature:—

"As stated in the Report of Council presented to the Society at the Anniversary Meeting, the President and Council offered to become the Publishers of the proposed International Catalogue, on behalf of the International Council, and to advance the capital sum needed to start the enterprise.

"I have now the pleasure of announcing that the International Council of the Catalogue, which met yesterday and to-day in the rooms of the Society, has accepted the offers of the Royal Society, and that this great undertaking, which has for several years engaged the earnest attention and demanded the continued labours of the Royal Society, as well as of other scientific bodies abroad and in this country, is now well on its way. The International Council has laid down all the necessary regulations, and prepared all the necessary instructions, for carrying out the task of collecting and editing, and it only remains for those who are taking part in the preparation of the Catalogue, to do their best to secure that the Catalogue shall fulfil the hopes which have been raised.

"It gives me great pleasure to make this announcement in the presence of several of our foreign brethren, whose co-operation has tended so much to the success of the enterprise."

The following Papers were read:—

I. "On the Spectrum of the more Volatile Gases of Atmospheric Air, which are not Condensed at the Temperature of Liquid Hydrogen. Preliminary Notice." By Professor S. D. LIVEING, F.R.S., and Professor JAMES DEWAR, F.R.S.

II. "Additional Notes on Boulders and other Rock Specimens from the Newlands Diamond Mines, Griqualand West." By Professor T. G. BONNEY, F.R.S.

III. "The Distribution of Vertebrate Animals in India, Ceylon, and Burma." By W. T. BLANFORD, LL.D., F.R.S.

IV. "Elastic Solids at Rest or in Motion in a Liquid." By C. CHREE, F.R.S.

The Society adjourned over the Christmas Recess to Thursday, January 17, 1901.

"On the Spectrum of the more Volatile Gases of Atmospheric Air, which are not Condensed at the Temperature of Liquid Hydrogen.—Preliminary Notice." By S. D. LIVEING, M.A., D.Sc., F.R.S., Professor of Chemistry, University of Cambridge, and JAMES DEWAR, M.A., LL.D., F.R.S., Fullerian Professor of Chemistry, Royal Institution, London. Received November 15,—Read December 13, 1900.

In August last some tubes were filled at low pressure by an improved process with the more volatile gases of the atmosphere.* The air was liquefied directly from that above the roof of the Royal Institution by contact at atmospheric pressure with the walls of a vessel cooled below - 200° C. When about 200 c.c. of liquid had condensed, communication with the outer air was closed by a stop-cock. Subsequently, communication was opened, through another stop-cock, with a second vessel cooled by immersion in liquid hydrogen, and a part of the liquid from the first vessel, maintained at − 210°, was allowed to distil into the second still colder vessel. When about 10 c.c. had condensed in the solid form in the second vessel, communication with the first vessel was cut off, and a manometer showed a pressure of gas of about 10 to 15 mm. of mercury.

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* In this paper we describe researches in continuation of those previously communicated to the Society by one of us, in a paper entitled "Application of Liquid Hydrogen to the Production of High Vacua, together with their Spectroscopic Examination," 'Roy. Soc. Proc.,' vol. 64, p. 231.

This mixture of gases was passed into tubes previously exhausted by a mercury pump, but before reaching the tubes it had to pass through a U-tube *immersed in liquid hydrogen so as to condense less volatile* gases, such as argon, nitrogen, oxygen, or carbonic oxide, which might be carried along by those more volatile. Previous trials with tubes filled in the same way, except that the U-tube in liquid hydrogen was omitted, showed that these tubes contained traces of nitrogen, argon, and compounds of carbon. The tubes filled with gas which had passed through the U-tube showed on sparking no spectrum of any of these last-mentioned gases, but showed the spectra of hydrogen, helium, and neon brilliantly, as well as a great many less brilliant rays of unknown origin. In addition, they showed at first the brightest rays of mercury, derived, no doubt, from the mercury pump by which they had been exhausted before the admission of the gases from the liquefied air. After some sparking the mercury rays disappeared, probably in consequence of absorption of the mercury by the electrodes, which were of aluminium.

In one experiment the mixture of gases in the second vessel, into which a fraction of the liquefied air was distilled as above described, was pumped out without being passed through the U-tube in liquid hydrogen and examined. This mixture was found to contain 43 per cent. of hydrogen, 6 per cent. of oxygen, and 51 per cent. of other gases—nitrogen, argon, neon, helium, &c.—and it was explosive when mixed with more oxygen. This shows conclusively that hydrogen in sensible proportion exists in the earth's atmosphere, and if the earth *cannot retain hydrogen or originate it, then there must be a continued* accession of hydrogen to the atmosphere (from interplanetary space), and we can hardly resist the conclusion that a similar transfer of other gases also must take place. The tubes containing the residue of atmospheric gases uncondensed at the temperature of liquid hydrogen we have examined spectroscopically.

On passing electric discharges through them, without any condenser in the circuit, they glow with a bright orange light, not only in the capillary part, but also at the poles, and at the negative pole in particular. The spectroscope shows that this light consists in the visible part of the spectrum chiefly of a succession of strong rays in the red, orange, and yellow, attributed to hydrogen, helium, and neon.

Besides these, a vast number of rays, generally less brilliant, are distributed through the whole length of the visible spectrum. They are obscured in the spectrum of the capillary part of the tube by the greater strength of the second spectrum of hydrogen, but are easily seen in the spectrum of the negative pole, which does not include the second spectrum of hydrogen, or only faint traces of it. Putting a Leyden jar in the circuit, while it more or less completely obliterates the second spectrum of hydrogen, also has a similar effect on the greater part of these other rays of, as yet, unknown origin. The violet and ultra-violet part of the spectrum seems to rival in strength that of the red and yellow rays, if we may judge of it by the intensity of its impressions on photographic plates. We were surprised to find how vivid these impressions are up to a wave-length 314, notwithstanding the opacity of glass for rays in that part of the spectrum. The photographs were taken with a quartz calcite train, but the rays had to pass through the glass of the tube containing the gases.

We have made approximate measurements of the wave-lengths of all the rays which are sufficiently strong to be seen easily or photographed with an exposure of thirty minutes, and give a list of them below. These wave-lengths are computed to Rowland's scale, and were deduced from the deviations produced by two prisms of white flint glass for the visible, and of calcite for the invisible, rays. The wave-lengths assigned to the helium lines are those given by Runge and Paschen, and some of these lines were used as lines of reference. In general, the iron spark spectrum was the standard of reference.

The tubes when first examined showed the lines of the first spectrum of hydrogen vividly, and the earlier photographs of the spectrum of the negative pole contained not only the violet lines of hydrogen, but also the ultra-violet series as far up as λ 377. In order to get impressions of the fainter rays, exposures of half an hour or more were required, and a succession of photographs had to be taken so as to get different sections of the spectrum into the middle of the field, where measurement of the deviations would not be impeded by the double refraction of the calc spar. As the light of the negative pole only was required, the electric discharge was made continuously in one direction only, with the result that the hydrogen lines grew fainter in each successive photograph, and soon disappeared altogether. Along with the ultra-violet rays, the less refrangible rays of hydrogen also disappeared, so that no trace of the C or F line could be seen, nor yet of the second spectrum, so long as the current passed in the same direction as before. Reversal of the current soon made the F line show again, so that it seems that the whole of the hydrogen was driven by the current to the positive pole. The conditions under which this ultra-violet series shows itself are a matter of interest. It appears here in the midst of a brilliant spectrum due to gases other than hydrogen, and yet it is very difficult to obtain a photograph of it when no gas but hydrogen is known to be present, or, at least, to become luminous in the electric discharge.

We have had an opportunity of comparing the spectrum of the volatile residue of air with that of the more volatile part of gas from the Bath spring. The tube did not admit of the separate examination of the light from the negative pole, but was examined end-on, so that the radiation probably included rays emitted from the neighbourhood of the negative pole. The whole of the hydrogen had been removed from the Bath gas, but not all the argon. In the spectrum of this gas the *rays of helium are dominant, decidedly stronger than those of* neon, although the latter are very bright. In the spectrum of the residue of atmospheric air, the proportion of helium to neon seems reversed, for in this the yellow neon line is as much more brilliant than *the yellow helium line as the latter is the more brilliant in the spectrum* of Bath gas. All the prominent lines in the spectrum of the volatile residue of Bath gas were also in that of the residue of atmospheric air, except the argon lines. There were, on the other hand, many lines in the latter not traceable in the former, some of them rather conspicuous, such as the ray at about λ 4664. It is, of course, probable that such rays are the outcome of some material not contained in the Bath gas. A very conspicuous pair of lines appears in photographs of the spectrum of the air residue, at about λ 3587, which is not traceable in the spectrum of Bath gas. The helium line, λ 3587·4, is seen in the latter spectrum, but is quite obscured in the former spectrum by the *great intensity of the new pair. This helium ray is really a close* double, with the less refrangible component much the weaker of the two, but the new pair are wider apart, and of nearly equal intensities; this character also distinguishes them from the strong argon line at λ 3588·6. They are, however, *very much more intense at the negative* pole than in the capillary, and it will require a good deal more study to determine whether these rays, and many others which we have not tabulated, are due to the peculiarity of the stimulus at the negative pole, *or to the presence of a previously unrecognised material.*

As our mixture of gases probably includes some of all such gases as pervade interplanetary and interstellar space, we early looked in their spectra for the prominent nebular, coronal, and auroral rays. Searching the spectrum about λ 5007 no indication of any ray of about that wave-length was visible in the spectrum of any one of the three tubes which had been filled as above described. Turning to the other green nebular line at about λ 4959, we found a weak rather diffuse line to which our first measure assigned a wave-length 4958. The correctness of this wave-length was subsequently verified by measuring with a micrometer eye-piece the distances of the line from the helium lines λ 4922·1 and λ 5015·7 which were in the field of view at the same time. The position of the line was almost identical with that of the iron spark line λ 4957·8, and the conclusion arrived at was that the wave-length was a little less than 4958, and that it could not be the nebular line. There remained the ultra-violet line λ 3727. Our photographs showed a rather strong line very close to the iron spark line λ 3727·8, but slightly more refrangible. As the line is a tolerably strong one, it could be photographed with a grating spectrograph along with the iron lines. This was done, and the wave-length deduced from measuring the photograph was 3727·4. This is too large by an amount which considerably exceeds the probable errors of observation, and we are forced to conclude that the nebular material is either absent from our tubes, or does not show itself under the treatment to which it has been subjected.

Although the residual gases of the atmosphere uncondensed at the temperature of liquid hydrogen do not show the nebular lines, we found that another tube gave a ray very close indeed to the principal green nebular ray. This tube had been filled with gas prepared in the same way as the others, with the exception that, in passing from the vessel into which the first fraction of liquid air was distilled, it was not passed through a U tube immersed in liquid hydrogen on its way to the exhausted tube. In consequence it contained traces of nitrogen and argon, and when sparked showed the spectra of these elements as well as those of hydrogen, helium, &c. The nitrogen spectrum disappeared after some sparking, but the tube still shows rays of argon as well as those of the gases in the other tubes. On examining *the spectrum of the negative pole in the neighbourhood of the principal* nebular green ray, a weak ray was seen in addition to those given by the other tubes. It was found by comparison with the nitrogen rays λ 5002·7 and λ 5005·7 to be a little less refrangible than the latter of these rays, and by measuring its distance from the nitrogen rays and from the two helium rays λ 4922·1 and λ 5015·7 with a micrometer eye-piece, the wave-length λ 5007·7 for the new ray was deduced. This looks as if we might find the substance which is luminous in nebulæ to be really present in the earth's atmosphere, and we hope shortly to be able to verify the observation of it.

Turning to the coronal rays, our tubes emit a weak ray at about λ 5304. This is not far from the wave-length λ 5303·7 assigned by Sir N. Lockyer to the green coronal ray. It is, however, greater than that assigned by Campbell, namely, 5303·26.[*] Other lines observed by us near the places of coronal lines are at wave-lengths about 4687, 4570, 4358, 4323, 4232, 4220, 3985, 3800. These are all weak lines except that at λ 4232, which is of tolerable strength, and that at λ 4220, which is rather a strong line. The wave-lengths 4323, 4232, 4220, and 3800 come very close to those assigned to coronal rays, but

[*] 'Astroph. J.,' vol. 10, p. 190.

the others hardly come within the limits of probable error. The ray 4220 seems too strong in proportion to the others, but the strength of that at 4232 seems to accord with the strength of the corresponding ray in the corona. It will be seen that the rays we enumerate above correspond approximately to the stronger rays in Sir N. Lockyer's list.* Further measures of the wave-lengths of the faint lines are needed before we can say definitely whether or no we have in our tubes a substance producing the coronal rays, or some of them.

As to the auroral rays, we have not seen any ray in the spectrum of our tubes near λ 5571·5, the green auroral ray. We have observed two weak rays at λ 4206 and λ 4198 which may possibly, one or both, represent the auroral ray λ 420. The very strong ray of argon, λ 4200·8, would make it probable that argon was the origin of this auroral ray, if the other, equally strong, argon rays in the same region of the spectrum were not absent from the aurora. Nor have we found in the spectrum of our tubes any line with the wave-length 3915, which is that of another strong auroral line. On the other hand it seems probable that the strong auroral line λ 358 may be due to the material which gives us the very remarkable pair of lines at about the place of N of the solar spectrum, λ 3587, which are very strong in the spectrum of the negative pole, but only faint in that of the capillary part of our tubes. It may well be that the auroral discharge is analogous to that about the negative pole. We have also a fairly strong ray at λ 3700, which may be compared to the remaining strong ray observed in the aurora λ 3700. This, however, is a ray which is emitted from the capillary part of our tubes as well as from the negative pole, and is, moreover, emitted by Bath gas, and may very likely be a neon ray.

We hope to pursue the investigation of this interesting spectrum, and if possible to sort out the rays which may be ascribed to substances such as neon and those which are due to one or more other substances. The gas from Bath, even if primarily derived from the atmosphere—which is by no means sure—seems to have undergone some sifting which has affected the relative proportions of helium and neon, and a more thorough comparison of its spectrum with that of the residual atmospheric gases may probably lead to some disentanglement of the rays which originate from different materials. The arrangement of the rays in series, if that could be done, would be a step in the same direction.

We are indebted to Mr. Robert Lennox, F.C.S., for the great help he gave us in the complicated manipulation with liquid hydrogen required to fill the spectral tubes, and to Mr. J. W. Heath, F.C.S., for kind assistance.

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* 'Roy. Soc. Proc.,' vol. 66, p. 191.

List of Approximate Wave-lengths of the Rays, Visible and Ultra-violet, observed about the Negative Pole.

The rays of hydrogen and helium, and those attributed to neon by other observers, are indicated by the chemical symbols of those substances:—

A "b" prefixed to the number expressing the wave-length indicates that the ray is emitted by gas from the Bath spring as well as by that obtained from the atmosphere

A "c" similarly prefixed indicates that the ray has been observed to be emitted from the capillary part of the tube as well as from about the negative pole

A "w" indicates a weak line, "s" a strong one, "d" a diffuse one, "vw" a very weak; and "vs" a very strong one

bc He	7281·8	bc He	5875·9		5031
	7247	vsbc Ne	5852·7	b He	5015·7
	7171	sb	5820	wd	4958
bc He	7065·5	sb	5804	b He	4922·1
vw	7058	sbc	5763	vw	4884
bc Ne	7034	bc	5747	sc H	4861·5
bc Ne	6931	bc	5718	vwb	4838
bc Ne	6716	bc	5689	vw	4819
bc He	6678·1	wbc	5662	vwb	4811
bc Ne	6601	bc	5656	wd	4791
sc H	6563		5592	wd	4754
bc	6535	b	5561	b Ne	4715
bc Ne	6506		5532	b Ne	4710
vwbc	6446	vwb	5503	b Ne	4704
bc Ne	6404	vw	5447	w	4687
bc Ne	6382	wb	5432	w	4680
bc Ne	6334	vw	5417		4664
bc Ne	6304	vwb	5409	wb	4657
bc Ne	6266	b Ne	5400	w	4647
bc	6244		5372	wb	4640
bc	6232		5360	w	4636
bc Ne	6217		5355	w	4628
b	6183	bc Ne	5341 a pair	w	4616
vwbc	6176	bc Ne	5330	w	4589
bc Ne	6163	w	5304	w	4583
bc Ne	6144	w	5298		4576
vwb	6128		5281	w	4570
b Ne	6097	b	5222		4540
b Ne	6075		5209		4538
b Ne	6031	b Ne	5204	w	4526
b	6001	b	5192	w	4523
b	5991	b Ne	5188	w	4518
wb	5987		5152	w	4508
bc Ne	5976	b Ne	5145	w	4500
wb	5964		5122	w	4488
sdb Ne	5945	b Ne	5116	vs He	4471·6
w	5919	b Ne	5080	vw	4460
w	5914		5074		4457
wb	5905	b He	5047·8	vw	4438
bc	5882	sbc	5038	wb He	4437·7

	4431	b ? He ?	3927	wbc	3510
	4429		3905	w	3504
	4424		3900	bc	3500
	4422	vsbc He	3888·8	bc He ?	3498
	4413	wbc He ?	3872		3482
	4409	bc He	3867·6		3481
	4398	w	3866	sbc	3473
	4392	w	3862	bc	3467
b He	4388·1	wc ?	3860	bc	3464
	4380	w	3856	bc	3460
w	4370	w	3842	w	3456
vw	4365	w	3840	bc	3454
w	4363	c H	3836	wc	3451
w	4358	b	3830	sbc He	3447·7
vw	4347	sbc He	3819·75	w	3429
s H	4340·7	wc He ?	3806	w	3424
w	4334	w	3800	sbc	3418
vw	4322	c H	3798	vw	3417
vw	4315		3777	w	3407
vw	4306	w H	3770	w	3404
w	4290		3768	b	3398
	4276		3754	b	3388
w	4270		3751	s	3378
w	4261	vw	3745	vw	3374
w	4258	w	3738	w	3372
	4251	? c	3735	bc	3370
	4241	? c	3728		3367
	4234	w	3722	w	3363
	4232	w	3721·5	vw	3362
	4220	s	3713		3360
w	4218		3710		3358
w	4206	bc He	3705·2	b He	3354·7
vw	4198	w	3703	s	3345
wc	4176		3701	w	3344
wb He	4169·1	s c ?	3694	s	3335
w	4151	c	3686		3329
b He	4148·9	c	3683		3327
w	4134	sc	3664	s	3324
b	4131		3655	sb	3319
wb	4128		3651	w	3315
b He	4121	w	3650	w	3313
w	4112		3644 a pair	w	3311
sc H	4102	sc He ?	3634 a pair		3310
w	4099	vw	3628	b He ?	3297
vw	4086	bc He	3613·8	vw	3254
w	4080	w	3609	wb	3250
w	4063	bc He ?	3600	s	3244
	4047	sbc	3593		3238
vw	4043	vs	3587·5 a pair	? pair	3230
vw	4037		3575		3225
vsbc He	4026·3	w	3571	s	3218
b He ?	4009		3569		3214
w	3996	w	3561		3209
vw	3985	w	3558		3199
vw	3980	vwb	3548	sb He	3187·8
sc H	3970		3543	w	3165
s He	3964·9	vscb	3521	w	3143
vw	3933	bc	3515		

"Additional Notes on Boulders and other Rock Specimens from the Newlands Diamond Mines, Griqualand West." By T. G. Bonney, D.Sc., LL.D., F.R.S., Professor of Geology, University College, London. Received November 21,—Read December 13, 1900.

The invasion of Griqualand at the beginning of the war with the Transvaal and Free State, put a stop, for a time, to working the Newlands Diamond Mines, some interesting specimens from which were brought to the notice of the Royal Society, on June 1st, in last year.* But shortly before the hurried departure of the employés, another small collection had been despatched to Mr. G. Trubenbach, the Managing Director in London, which he showed to me, early in the present year, most kindly placing the new specimens at my disposal for study. Some represented boulders, some the diamantiferous breccia, popularly called "blue ground,' in which these occur, some the "country-rock.' The first, though (so far as can be seen) without diamonds, include at least four additional species of rock; the second throw a little more light on the past history of the matrix. Moreover, they come from a new set of workings to the north-east of the former, where a shaft has been sunk, and galleries driven at a depth of about 465 feet. Apparently two "pipes" are connected by a narrow fissure filled with breccia.† So I have ventured to communicate the result of my investigations to the Royal Society, including with them a short note on a residue obtained by Sir William Crookes, F.R.S., after dissolving away almost the whole of a small fragment of the remarkable diamantiferous eclogite which was described in June, 1899.

(1.) *The Boulders.*

(*a.*) Of these one is rudely semi-oval in outline, measuring about 3½ inches in greatest length and breadth, and 1½ inch thick, being probably a piece broken from an ellipsoidal pebble. The rock is holocrystalline, composed chiefly of a pyroxene resembling bastite and of olivine, converted on the older-looking surfaces into a pale-green serpentinous material. Examination of a thin slice shows the rock to consist mainly of olivine, which exhibits incipient serpentinisation along cracks in the usual manner, and of a very pale brownish-green bastite, with one close cleavage; and possibly one or two small grains of a monoclinic pyroxene; spinel, and even original iron oxide, being apparently absent. Specific gravity, 3·074.

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* 'Roy. Soc. Proc.,' vol. 65, p. 223.

† The precise depth at which the specimens were obtained cannot be given, as the labels became illegible in the hurried transit.

(*b.*) Another specimen, apparently about half of a fairly well-worn boulder, is not quite so large. Under the microscope it is found to be practically identical in composition, but a little more serpentinised; a clear isotropic mineral sometimes forming a border to the enstatite. The presence of any original grains of iron oxide is doubtful, but one or two of augite can be recognised. Both specimens, however, may be named Saxonites.

(*c.*) Not very much worn, and rather triangular in shape, about 3 inches by 1 inch, and about 0.6 inch thick, consisting apparently of garnet, two pyroxenes and perhaps olivine. Microscopic examination shows olivine, almost wholly converted into serpentine, enstatite partially changed to another (the usual) variety of the same mineral, chrome diopside, a little colourless augite, with a diallagic habit, and pyrope (two specimens). As the last-named mineral is not abundant, the rock is more nearly related to the Lherzolites than to the Eulysites, and so may be named a granatiferous Lherzolite.

(*d.*) A roundish flat slab about 3·5 × 2·5 × 0·6 inches, containing red garnets, enstatite, and a bright green pyroxene. Microscopic examination shows olivine, partly converted into a dull yellowish-green serpentine, chrome diopside, some enstatite, now altered to a serpentine, the colour suggesting that it is chromiferous, and pyrope (not abundant). A little pale brown mica, probably secondary, occurs about the garnets and the diopside, in one case occupying a crack. The rock belongs to the granatiferous peridotites, and though it contains less enstatite than the last one, may also be regarded as a variety of Lherzolite.

(*e.*) The next specimen is evidently a fragment, the angles and edges of which have been slightly worn, as if by water. It measures about 3½ by 3 inches, and 1¼ inch in thickness. The rock in the fresher part consists of pyrope, and two minerals of a dull-green colour, but about half of one surface is affected by decomposition, which has penetrated to a depth of about ¾ inch. Here one of the pyroxenic minerals appears to be a pale-coloured bastite with the usual metallic lustre; the other of a brighter green tint. Examination with the microscope shows the following minerals:—(1) Olivine in various stages of conversion into serpentine; some grains being traversed as usual by very pale-green strings of the latter mineral, others completely changed into it, and of a yellowish or brownish colour; minute dark-brown needles are sometimes present (? rutile). (2) Bastite with a well-developed pinacoidal cleavage, sometimes partially or even wholly converted into a fibrous material, which with transmitted light is a rather rich green colour, the usual small brown negative crystals being developed in some grains. (3) A very pale sea-green augite, probably a chrome diopside. (4) Pyrope; the grains having a kelyphite rim and showing incipient mineral change along the cracks.

As one or two grains with a general resemblance to the bastite appear to give an oblique extinction, a third pyroxene may be present in very small quantities. Mica is wanting as an original constituent, and most of the iron oxide is secondary, but one grain may be primary. The structure of the rock is granular, no constituent being idiomorphic; hence the order of consolidation cannot be determined with certainty, but I incline to placing the olivine, which is slightly the most abundant mineral, first, and the garnet, which is slightly the least so, last. The rock is distinguished from ordinary Eulysite by the presence of a fair amount of an enstatite. But as this does not indicate any important difference in chemical composition, I prefer calling it Enstatite Eulysite to burdening petrology with a new name.

(*f*) This specimen has a rude resemblance to an oven bottom loaf, measuring full 16 inches in two directions at right angles on the curved surface, the flat side being probably the result of a fracture, apparently produced after most of the rounding had been done. The rock is holocrystalline, its principal constituents being dull red garnets and green pyroxenes. The former have their outer surface worn smooth and flat, the latter a very slightly corroded one. The rock is macroscopically identical with the eclogite described in the last paper, and it proves to be composed of pyrope and chrome diopside with occasionally a few fibres of secondary hornblende, no grain either of olivine or iron oxide occurring in the slice.

(*g*) This specimen is a rudely trapezoidal block with rounded edges and corners, measuring about 2½ inches each way, apparently rather water worn, consisting of somewhat rounded crystals of greenish pyroxene over an inch in length, in a matrix of a similar mineral and felspar. Specific gravity, 3·125. On examination with the microscope, the larger grains prove to be generally diallage, a faint sea-green in colour, with a close pinacoidal cleavage, often made more distinct by the deposit of a little opacite or ferrite. Small brown negative crystals are frequent, one of their longer edges lying parallel with an axis of elasticity. This mineral is altered locally into a pleochroic hornblende (changing from a straw to a burnt umber tint). The diallage is sometimes bordered by, and near its edges occasionally encloses, small grains of a slightly browner and more pleochroic mineral, extinguishing parallel with its principal cleavage, and thus representing a rhombic pyroxene,* but it also throws out root-like prolongations in which a cross cleavage is visible. Where the diallage has been replaced by hornblende, the latter often extends some little distance into the roots, which in a few cases suggest the presence of the rhombic constituent. These are embedded in felspar, thus affording a pegmatitic structure which varies in different parts of the slices from incipient to well developed (see figure). Some of the larger diallage crystals also show a curious structure with crossed nicols; small lamellæ of a different tint, arranged in a kind of network with lozenge-shaped meshes, making

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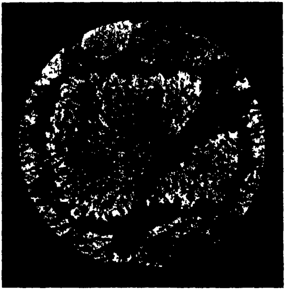
* These locally are seen to pass into a yellowish serpentinous mineral which with crossing nicols shows a fibrous structure and fairly bright polarisation tints.



Pegmatitic association of a pyroxene and felspar (composite). × 21. The "rootlets" and most of the mineral round the central grain is pyroxene Decomposition shown about a crack.*

their appearance. These possibly may indicate an early stage of the conversion of the diallage into hornblende The grains of felspar vary much in size, even when associated with the "rootlets" of pyroxene. They are generally in good preservation; exhibit twinning, usually on the albite type, and are shown by the extinction angles to be mostly if not wholly labradorite. Small grains of iron oxide are present, which are most abundant near the margin of the larger pyroxenic grains. They are sometimes scattered in the pegmatite, and in one or two cases are slightly root-like in shape. Cracks traverse the rock and have led to mineral change. They are often lined with small crystals of a brown mica, similar to that which occurs in some specimens of the "blue ground." These are imbedded in a rather earthy-looking granular material, which is, no doubt, a decomposition product from the felspar. Pegmatitic structures, whether macroscopic or microscopic, are fairly common in granites, where the associated minerals are quartz and felspar, but, so far as my experience goes, are infrequent with other minerals.

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* I am indebted to my friend Mr Coomára-Swámy for the microphotograph.

Professor Rosenbusch however mentions the occurrence in some syenites (including those with elæolite) in diorite (very rare) and in a hyperstheme gabbro (or norite) from Ekersund on the west coast of Norway and St Paul's Island Labrador.* Thus we may be content to call this rock a pegmatitic hornblendic Gabbro.

(*h*) This perhaps represents a pebble rather than a boulder, for it is a fragment only about 1½ × 1½ × 1 inches adhering to a piece of 'blue ground' the surface in contact with the latter being well rounded. Macroscopically it appears to be a medium grained diorite: the microscope shows a holocrystalline granular structure, the plagioclastic felspar is in fair preservation and perhaps is labradorite, the hornblende is rather strongly pleochroic ranging from pale brownish green to deep brown. The mineral however is not original, but an alteration product from a pale green augite (omphacite?). Grains of iron oxide are also present. Slight decomposition has taken place in a narrow zone from the surface inwards.

(*i*) The last specimen is a lump of irregular shape. Presumably it is from the blue ground, but there is nothing to prove this. In a compact dark brown to slightly purple groundmass a number of irregularly formed greenish grey patches are scattered so as to suggest flow brecciation. These when examined under the microscope, are a very light greyish brown in colour exhibiting flow structure, minute devitrification and some decomposition. The matrix is darker, sprinkled with opacite and ferrite minutely devitrified showing an irregular wavy structure and occasionally ill-defined crystallites of plagioclase felspar. The rock now a felsite or porphyrite was probably once either a sanidine trachyte or more probably an andesite, with flow brecciation. This specimen possibly may not represent a boulder, but a dyke or flow associated with the blue ground.

(2) *Dimartifeins Mine*

Specimens of the "blue ground" in which the boulders occurred were also sent. As they came from another part of the mine, and the best preserved exhibited one or two slight differences I have had a few slices prepared. To the unaided eye the matrix is more of a purple brown colour, slightly more compact and hard, but more brittle, the fragments of magnesian minerals, however, seeming more completely serpentinised. A few small, rather crumbling, rock fragments, of a dull white colour, speckled with green, are present. Microscopic examination shows that the larger minerals do not call for any special notice, except that a rudely crescentic pyrope has a kelyphite rim on the concave as well as on the convex side, proving the fracture to be an old one. But the small plates of a brown mica, the occurrence of which has been already noticed,[*] are very abundant in the matrix. These plates in some of the specimens are rather irregularly outlined, and rarely exceed 0·001 inch in diameter, but in others they average about double that size, and occasionally a few of them may even exceed 0·004 inch. Then the outline is more rectangular, and the cleavage more distinct. The smaller flakes often tend to form a zone around included rock fragments, and scattered granules of iron oxide seem more common in the slices containing the larger flakes.[†] I have now no doubt that the mineral is a secondary product.

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* Elemente der Gesteinslehre (1898) p 221. A case where the structure is more like that of the true graphic granite, from the dolerite of Pouk Hill is described by Mr Allport Quart Journ Geol Soc vol 30, p 549 and figured by Mr Teall British Petrography Pl XXIII fig 2. An instance of micrographic intergrowth of quartz and calcite is described by Mr Coomara Swámy in the aforenamed journal vol 56 pp 605 606

The unusual abundance of a minute brown mica in the ground mass made an analysis desirable. For that annexed I am indebted to Mr. C. James, who has executed it in the laboratory at University College under the supervision of Professor W. Ramsay.

Silica	38·77
Alumina	14·62
Ferric oxide	11·36
Calcium oxide	4·51
Magnesia	12·14
Potash	2·63
Soda	1 90
Loss on heating CO_2 and H_2O.	13 55
	99·48

(The iron was all estimated as Fe_2O_3; one specimen gave a trace of nickel.)

If we compare this analysis with one given by Professor C. Lewis,[‡] $SiO_2 = 33·00$, FeO (including Al_2O_3) = 12·00, $MgO = 32·38$, $CaO = 0·63$, $Na_2O = 0·67$, $CaCO_3 = 16·02$, $H_2O = 6·0$ (total 101·71), and with those of Kentucky "kimberlites" quoted by Rosenbusch,[§] and by Lewis,[‖] we see them to be poorer in alumina and alkalies, but richer in magnesia. Serpentine, in fact, forms the dominant silicate in them, a ferro-magnesian mica in this, the other mineral not amounting at most to a quarter of the whole rock. But we must remember that some of the deeper-seated specimens from the De Beers Mine, though belonging to the typical mass of kimberlite, are hardly less rich in the secondary mica than this one from Newlands, so that from the chemical point of view, apart from other considerations, the propriety of classing "kimberlite" with the (altered) peridotites is worse than doubtful.

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[*] 'Geol. Mag.,' 1897, p. 431.

[†] *In all these specimens from Newlands opaque granules (? ilmenite) seem to take the place of the translucent brownish granules (in part perofskite) in the specimens from De Beers Mine.*

[‡] 'The Matrix of the Diamond,' p. 47.

[§] 'Elemente der Gesteinslehre,' p. 165.

[‖] 'The Matrix of the Diamond,' p. 64, *cf.* p. 61.

The rock fragments are often about 0·1 inch in diameter, though pieces nearly half an inch across also occur, sub-angular to rounded in form. The majority represent varieties of basalt, some apparently retaining traces of a glassy base, others rather minutely holocrystalline. *They show signs of alteration, but nothing in their structure or* composition calls for any detailed description. It is difficult to determine the exact nature of the light-coloured fragments. The green mineral is sometimes a granular augite, rather decomposed, sometimes an actinolite; the lighter (dominant) part often effervesces briskly with HCl, and calcite is seen under the microscope, associated with a grey decomposition product, which often suggests the former presence of a felspar. In one case a holocrystalline granular structure is clearly seen, and the replacing mineral has some resemblance to pseudophite. Hence I consider these fragments to represent a plagioclase-augite rock allied to gabbro, and related to the boulder already described. Its presence, and the comparative abundance of bits of basaltic rock, seem to be characteristic of the locality.

One specimen, however, calls for a little notice. Part of it resembles a compact mudstone; the rest, about an inch across, is rather decomposed blue ground; the outer side of this suggesting that, as in the case of a specimen described last year, the "blue ground" may occupy a fissure in the "country rock." On microscopic examination, however, this proves to be doubtful. The apparent outer surface is only a vein product, consisting of a fibrous mineral, possibly arragonite, associated with a little actinolite. The seeming mudstone is more like a very decomposed igneous rock, probably a rather felspathic basalt. The "blue ground" is also much decomposed, the mineral fragments being converted into a pale greenish-yellow fibrous material, much of which is actinolite. A fragment of basalt (not identical with the other) appears to be altered for a depth of not quite one-tenth of an inch from the exterior, for in this part small distinct flakes of brown mica are scattered about.

The specific gravity of a specimen of "blue ground," rather harder than the rest, was 2·667; two others, representing the most brittle variety, were weighed, but as each crumbled a little when immersed, the results are slightly too low. One (the better) was 2·622, the other 2·614. I tried the former a second time, but as it broke up more readily than before, abandoned the attempt.

I referred last year to a pyrope in which diamonds were embedded.

Another specimen has now been found. The pyrope apparently was rounded in form, about a ¼ inch in diameter, and surrounded by a kelyphite rim It is broken across, thus disclosing the diamond, an octahedron, only one face of which is completely exposed. This is slightly stepped, and measures roughly one-tenth of an inch along the edge A small piece of the usual purplish breccia adheres to the pyrope, so the case is exactly parallel to the former one. In each the perfect form of the diamond shows that it crystállised before the garnet, and as the ordinary varieties of the latter mineral seem to be produced at a high temperature,* the association may be significant

(3.) *The Country Rock*

A few specimens of this were also sent, but only two varieties present any feature of interest One is a greenish conglomerate with calcareous matrix, and rounded pebbles up to about ¼ inch diameter; the other has a pale-grey matrix, speckled with some small angular dark-green fragments and a few sub angular pebbles up to about an inch in diameter; one apparently a red felsite, the others diabase. In the first specimen the microscope shows abundant sub-angular to rounded grains, mostly diabase, of which there are at least half a dozen varieties, a microgranite and two or three rocks more fragmental in aspect, one perhaps a tuff, another apparently a quartzite, affected by pressure, and a third a sub-crystalline dolomite These are cemented by microgranular calcite, containing probably a little magnesia (the crystals often forming a kind of border to the rock fragments), the interspaces being filled in with clear dolomite. In this cement are embedded some angular bits of quartz, a fragment of altered felspar, and one or two, perhaps, chalcedony. The other specimen shows a fine-grained muddy matrix, in which are scattered angular to sub-angular grains of quartz, with a little decomposed felspar, a little of a green mineral (? replacing pyroxene), decomposed iron oxide, and perhaps some small rutiles, with rock fragments, generally rather rounded, representing compact diabase, or possibly sometimes andesite, and one or two of a sub-crystalline limestone. Both these specimens, as Mr Trubenbach informs me, represent a rock named "bastard blue" by the miners, and it has been pierced in both shafts, the diamantiferous breccia, or "blue ground," apparently passing under it in the second shaft. As, however, there is no real relationship between the two rocks, I regard the association as fortuitous.

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* I do not forget the remarkable garnets from the Bastogne, described by Professor Renard ('Bull. du Musée Royal de Belgique,' vol. 1, p. 9), or that called pyreneite (also one of the andradite group), but both these minerals are very abnormal. [The genesis of the former is discussed by Miss C. A. Raisin, D.Sc., in a paper which will appear in the 'Quarterly Journal of the Geological Society' for 1901.]

(4.) *Residue from the Diamantiferous Eclogite*

After the reading of my description of the "diamantiferous eclogite," Sir W. Crookes kindly offered to examine that rock for microscopic diamonds. Taking one of the fragments, weighing 130·5 grammes, which had been detached in slicing the specimen, he treated it as follows :—"After being very coarsely broken up, the material was put into a very strong sulphuric acid. The acid was boiled for some time, and, after being allowed to cool, the residue was washed, dried, and then heated for some hours in strong hydrofluoric acid. After it had been well washed and dried the treatment with hot sulphuric acid was repeated. The mass, after a few alternations of these acids, became disintegrated, and all, except a few crystalline lumps, were dissolved. After about ten treatments only a few small crystals remained, and these (with the exception of a sample) were reduced by a few more boilings with the acids to a single small one about half a millimetre in diameter." This was boiled fourteen times in each acid, and appeared to be slightly reduced in size. "It sinks in methylene iodide, specific gravity 3·35." This was sent to me with some of the small crystals just mentioned, all being mounted. The solitary survivor of the whole treatment showed on one side curved crystal faces, but on the other appeared imperfect. These faces, so far as I could judge, indicated an isometric or possibly a rhombohedral mineral. Its refractive index is high, the colour a pale smoke-brown, and it apparently produced some effect on polarised light. That, however, was not conclusive, for diamonds from Newlands, as at Kimberley, are often in such a state of strain as to be anisotropic. Of the survivors of the first treatment, the more abundant were colourless, rough in outline, but possibly showing one cleavage surface, apparently at right angles to an optic axis, polarisation tints bright; the refractive index high, but inferior to that of a diamond. It appeared to me not improbably corundum. The less abundant granules were more rounded in outline, with rather rough, possibly corroded, surfaces, translucent, of a resin-brown colour, apparently producing some effect on polarised light; on the whole they seemed to bear some resemblance to rutile. But to come to any conclusion about the first mineral it was necessary to detach it from the mount. As I have no apparatus for very delicate work, that not coming within my usual line of study, I had recourse to Mr. L. Fletcher, the Keeper of Mineralogy, and Mr. L. J. Spencer, also of that Department, at the British Museum. The latter attempted to measure the supposed diamond with the goniometer; the faces, however, were too curved for the purpose, but both of them regarded the edges as too sharp for the mineral to have suffered appreciably from the acid, as Sir W. Crookes was inclined to think. They consider it to be really isotropic, and a diamond.[*] Mr. Spencer thinks that the colourless birefringent fragments are perhaps optically uniaxial, and that they may very well be corundum.[†] The browner grains he suggests are also diamonds. In favour of this identification is the fact that small diamonds occur at the Newlands Mine (I have seen some in Mr. Trubenbach's hands), rather ovoid in shape, with a roughened surface, some a yellowish-brown, some colourless. But against it we may urge that they appear to have been destroyed during the second treatment.[‡] Be this as it may, Sir W. Crookes has succeeded in showing that microscopic diamonds do occur in the eclogite, which contains those of larger size.

To conclude: in addition to this residue from the eclogite we have ascertained (1) the existence, in some quantity and variety, of pre-triassic diabase,[§] (2) the abundant development of a microscopic brown mica in the ground mass of the so-called kimberlite; (3) the presence in it, as true boulders, of at least four more species of holocrystalline rock. The last fact acquires an additional importance, because, since the publication of my former paper, the boulders therein described have been claimed as "concretions" in the so-called kimberlite.[‖] With this matter I have dealt elsewhere,[¶] but the identification of seven species or strongly-marked varieties of holocrystalline rocks, peridotites, eclogites, &c., in which the minerals at the surface are worn as if by the action of water, not to mention the general structure of the so-called kimberlite, must, I think, offer insuperable difficulties even to the most enthusiastic advocate of concretionary action.

"The Distribution of Vertebrate Animals in India, Ceylon, and Burma."

By W. T. BLANFORD, LL.D., F.R.S.

Received December 3,—Read December 13, 1900.

(Abstract.)

Several contributions on the subject of the distribution of Vertebrata, or geographical Zoology, in India and the neighbouring countries have been made by Elwes,[*] von Pelzeln,[†] Wallace,[‡] Sharpe,[§] Newton,[||] Gadow,[¶] Lydekker,[**] and W. Sclater,[††] besides the present author.[‡‡] The majority of these contributions deal, however, with birds or mammals alone, the first-named class having received the greatest amount of attention.

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[*] On re-examining the specimen, now that Mr. Spencer has kindly mounted it in a better position, I agree with this determination.

[†] On a final examination of the slides, I find among them one if not two small grains which I strongly suspect to be diamonds.

[‡] A final examination and comparison with some bits of "bort" given me by Mr. Trubenbach has not made me more favourable to my original identification with rutile.

[§] That is, at any rate, older than the time when the Karoo series was deposited.

[‖] Professor Beck, 'Zeitschrift für Praktische Geol.,' December, 1899.

[¶] 'Geol. Mag.,' 1900, p. 246.

The completion of the seven volumes containing descriptions of all the Vertebrata, in the 'Fauna of British India,' affords an opportunity of reviewing generally the distribution of terrestrial vertebrate animals throughout the British possessions in India, Ceylon, and Burma. The limits are those of the British Indian territories and dependencies with the addition of Ceylon (which, although British, is not under the Indian Government) Baluchistan, all the Kashmir territories (with Gilgit, Ladak, &c.), Nepal, Sikhim, Bhutan, and other Cis-Himalayan States, Assam, Manipur, the Burmese Shan States, Karennee, and the Andaman and Nicobar Islands are included, but not Afghanistan, Kashgaria, Tibet, Yunnan, Siam, or the Malay Peninsula south of Tenasserim.

For the study of zoological distribution there are few, if any, regions on the earth's surface that exceed British India and its dependencies in interest The area is about 1,800,000 square miles, and although the vertebrate fauna is by no means thoroughly explored, it is well known throughout the greater part of the area and fairly known throughout the whole, better probably than in any other tropical and sub-tropical tract of approximately equal extent. The variety of climate is remarkable, within the area are included the almost rainless deserts of Sind and the locality on the Khási Hills distinguished by the heaviest rainfall known, the cold arid plateau of the Upper Indus drainage, and the damp tropical forests of Malabar and Tenasserim. The country is bounded on the north by the highest mountain range in the world, and on the south by an ocean extending to the Antarctic regions Another element of interest lies in the fact that the peninsula of India is a land of great geological antiquity, there being no evidence that it has ever been submerged, although the greater part of the Himalayas and Burma have at times been beneath the sea.

The plan adopted for the study has been to divide the whole country into nineteen tracts, distinguished by physical characters such as rainfall, temperature, presence or absence of forests, and prevalence of hilly ground, and to construct tables showing the distribution of each genus of land or fresh-water vertebrate in the tracts. Genera have been selected for consideration, because families and sub-families are too few in number and too wide in range, whilst species are too numerous and too unequal in importance. It is recognised that there is much difference in the value of genera in different groups, the generic differences in passerine birds, for instance, being as a rule of inferior rank to those in some other orders of birds, or to those generally adopted amongst mammals, reptiles, and batrachians. In the demarcation of regions and sub-regions, terrestrial mammalia are regarded as of primary importance.

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[*] 'P. Z. S.,' 1873, p. 645

[†] 'Africa-Indien,' "Verh. Z.-B. Ges. Wien," 1875, p 62.

[‡] 'Geographical Distribution,' vol. 1, pp 81, &c., 1876.

[§] 'Natural Science,' August, 1893. p 108.

[||] 'Dictionary of Birds,' p. 358 (1893).

[¶] Bronn's 'Kl. Ord. d. Thierreichs,' VI, 4, Vogel, p. 296 (1893).

[**] 'Geographical History of Mammals,' p. 266 (1896).

[††] 'Geographical Journal,' 1896, vol. 8, p. 380, 'Geography of Mammals,' p. 131.

[‡‡] 'Jour. As. Soc. Beng.,' vol. 39, pt 2, p 336 (1870), 'A. M. N. H.' (4), vol. 18, p. 277 (1876); Introduction to "Mammalia," 'Fauna Brit. Ind.,' p. IV (1888).

The tracts are the following:

A. *Indo-Gangetic Plain.*

1. Punjab, Sind, Baluchistan, and Western Rajputana.
2. Gangetic Plain from Delhi to Rajmahal.
3. Bengal from Rajmahal to the Assam Hills.

B. *Indian Peninsula.*

4. Rajputana and Central India as far south as the Nerbudda.
5. Deccan from the Nerbudda to about 16° N. lat. and from the Western Ghats to long. 80° E.
6. Behar, Orissa, &c., from the Gangetic Plain to the Kistna.
7. Carnatic and Madras, south of 5 and 6, and east of the Western Ghats.
8. Malabar Coast, Concan, and Western Ghats or Sahyadri range from the Tapti River to Cape Comorin.

C. *Ceylon.*

9. Northern and Eastern Ceylon.
10. Hill Ceylon, the Central, Western, and Southern Provinces.

D. *Himalayas.*

11. Western Tibet and the Himalayas above forest.
12. Western Himalayas from Hazara to the western frontier of Nepal.
13. Eastern Himalayas, Nepal, Sikhim, Bhutan, &c.

E. *Assam and Burma.*

14. Assam and the hill ranges to the south, with Manipur and Arrakan.
15. Upper Burma, north of about 19° N. lat.
16. Pegu from the Arrakan Yoma to the hill ranges east of the Sittang.
17 Tenasserim as far south as the neighbourhood of Mergui.
18. South Tenasserim, south of about 13° N. lat.
19. Andaman and Nicobar Islands.

A review of the fauna of these tracts leads to the following conclusions: -

I. The Punjab tract differs greatly in its fauna from the Indian peninsula and from all countries to the eastward, so greatly that it cannot be regarded as part of the Indo-Malay or Oriental region. Of terrestrial mammals, bats excluded, 30 genera are met with, of which 8 or 26⅔ per cent. are not Indian, whilst of reptiles (omitting crocodiles and chelonians) 46 genera occur, and of these 20 or 43½ per cent. are unknown further east. Of the corresponding orders of mammalia 46, and of reptiles 80 genera occur in the Peninsula, and 24 or 52 per cent. of the former and 57 or 64 per cent of the latter are not found in the Punjab tract. The differences would be larger but for the fact that certain genera, for instance, *Antilope* and *Boselaphus* (nilgai), are found east of the Indus though not further west, and that a few Indian species straggle into the Punjab area. All the genera met with in the Punjab tract and wanting farther east are either Holarctic forms or peculiar, but with Holarctic affinities.

The Punjab, Sind, and Western Rajputana are in fact the eastern extremity of the area known as the Eremian or Tyrrhenian or Mediterranean sub-region, generally regarded as part of the Holarctic region, but by some classed as a region by itself corresponding to the Sonoran in North America.

II. The Himalayas above the forests and such portions of Tibet as come within Indian political limits (Gilgit, Ladák, Zanskar, &c.) belong to the Tibetan sub-region of the Holarctic region. Of twenty-five mammalian genera hitherto recorded from No. 11 (the Tibetan) tract, 11 or 44 per cent. are not found in the Indo-Malay region. That Tibet forms a distinct mammalian sub-region has already been shown in other papers.*

III. India proper from the base of the Himalayas to Cape Comorin, and from the Arabian Sea and the eastern boundary of the Punjab tract to the Bay of Bengal and the hills forming the eastern limit of the Gangetic alluvium, should, with the addition of the island of

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* 'Geol. Mag.,' 1892 (3), vol. 9, p. 164; 'P. Z. S.,' 1893, p. 448.

Ceylon, be regarded as a single sub-region, and may be conveniently entitled the Cisgangetic sub-region.* The forests of the Sahyádri range and of the Western or Concan and Malabar coast and the hill area of Southern Ceylon have a far richer fauna than the remaining area, but are not sufficiently distinct to require sub-regional separation.

The hill fauna of the Sahyádri range, especially on the highest portions, such as the Nilgiri and Anamalai Hills, and that of the hill group in South-western Ceylon, contain several Himalayan genera and species, but not sufficient to enable the S. Indian and Ceylonese areas to be classed with the Himalayan forest area in a separate sub-division or sub-region.

The Cisgangetic sub-region is distinguished from the Transgangetic by the presence amongst mammals of Hyænidæ, Erinaceinæ, Gerbillinæ, of three peculiar genera of Antelopes and of some other types; amongst birds by the occurrence of Pterocletes (sand grouse), Phœnicopteri (flamingoes), Otididæ (bustards) and Cursoriinæ; amongst reptiles by the possession of the families Eublepharidæ, Chamæleontidæ and Uropeltidæ, together with many peculiar Geckonidæ, Agamidæ and Lacertidæ, and amongst batrachians by about one-half of the genera found in each sub-region being absent in the other. The difference between the reptiles and batrachians by itself would justify the classification of the two areas as distinct regions, a view adopted by several writers.

The following figures show the total number of genera recorded from the Cisgangetic sub-region and the percentage of them not ranging into the Transgangetic area, the Himalayas and Burma.

	Cisgangetic	Not Transgangetic.	
Mammals	62	14 or 22·5 per cent.	
Birds	347	46 or 13	,,
Reptiles ..	93	39 or 42	,,
Batrachians .	17	9 or 53	,,
Freshwater fishes .	58	9 or 15·5	,,

Omitting bats, the number of Cisgangetic mammalian genera is forty-six, of which 14 or 30 per cent are wanting in the Himalayas and east of the Bay of Bengal.

The difference between the Cisgangetic vertebrate fauna and that inhabiting the rest of the Indo-Malay or Oriental region is partly due to the absence in the former of numerous Eastern types, and partly to the presence of two constituents besides the Oriental genera, which, especially in forest, form a majority of the animals present. One of these two constituents consists of mammals, birds, and reptiles having a distinct relationship with Ethiopian and Holarctic genera, and with the Pliocene Siwalik fauna. This constituent of the Cisgangetic fauna it is proposed to distinguish by the term Aryan The other constituent is composed of reptiles and batrachians, and may be termed the Dravidian element The latter is well developed in the south of the Peninsula and especially along the south west or Malabar Coast, and in Ceylon, but it gradually disappears to the northward, its northern limit, so far as is known at present, not extending to the 20th parallel of north latitude It is probable that this is the oldest part of the Cisgangetic fauna, and it may have inhabited the country since India was connected by land with Madagascar and South Africa, across what is now the Indian Ocean, in Mesozoic and early Cenozoic times. The other two elements, the Indo-Malay or Oriental and the Aryan, are probably later immigrants, and its wider diffusion may indicate that the Oriental element has inhabited the Indian Peninsula longer than the Aryan has. There appears some reason for regarding the Oriental portion of the fauna as dating in India from Miocene times and the Aryan from Pliocene, whilst in the Pleistocene epoch the proportion of Aryan to Oriental types of mammals in India, as shown by the fossil faunas of the Nerbudda and the Karnul Caves, was much larger than at the present day

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* The terms "Cisgangetic" and "Transgangetic" have already been employed by Professor Gadow, *l.s.c.*

There are some other peculiarities of the Indian Peninsular fauna to which attention may be called. One of these is the presence of genera and sometimes of species which are found on both sides of the Bay of Bengal, but not in the Himalayas or Northern India. A good example is afforded by the genus *Tragulus*, of which one species inhabits Ceylon and India south of about 22 N lat whilst two others are found in Southern Tenasserim and the Malay Peninsula In Pliocene times, the genus inhabited Northern India Another instance is the lizard *Ladepes guttatus* found in Burma and Arrakan, and also in South Canara on the West Coast of India. Examples amongst reptiles are rather numerous. Moreover, whilst there are numerous alliances between the animals of Peninsular India and those of Africa, there are also some curious connections between India and Tropical America, but these are chiefly amongst invertebrates. Some, however, are found in reptiles. It is probable that such Indo-American connections are vestiges of older life than the Indo-African. They are of course, generally speaking, instances of animal groups once more widely distributed, but now only preserved in a few favourable tropical localities.

IV. The forest area of the Himalayas belongs to the same sub-region as Assam, Burma (except South Tenasserim), Southern China, Tonquin, Siam, and Cambodia, and to this sub-region the term Transgangetic may be applied. It is distinguished from the Cisgangetic sub-region by the absence of the animals already specified as characteristic of that

area and by the presence of the following, which are wanting in the Indian Peninsula—Mammals. the families Simiidæ, Procyonidæ, Talpidæ, and Spalacidæ, and the sub-family Gymnurinæ, besides numerous genera, such as *Prionodon*, *Helictis*, *Arctonyx*, *Atherura*, *Nemorhaedus*, and *Cemas*. Birds: the families Eurylæmidæ, Indicatoridæ, and Heliornithidæ, the sub-family Paradoxornithinæ. Reptiles. Platysternidæ and Anguidæ. Batrachians Discophidæ, Hylidæ, Pelobatidæ, and Salamandridæ.

The following are the numbers of the genera in the different classes recorded from the Indian portion of the Transgangetic region, but not from the Cisgangetic

	Transgangetic	Not Cisgangetic	
Mammals . . .	74	26 or 35	per cent.*
Birds	475	174 or 36·5	,,
Reptiles ...	84	30 or 35·5	,,
Batrachians	16	8 or 50	,,
Freshwater fishes	67	18 or 27	,,

Omitting bats, the number of Transgangetic mammals within Indian limits are fifty-four, of which 22 or 40 per cent. are not Cisgangetic.

The relations of the Himalayan fauna to that of Assam and Burma on the one hand and to that inhabiting the Peninsula of India on the other may be illustrated by the mammals with bats omitted. Of forty-one genera occurring in the Himalayas, three are not found in the hills south of Assam or in Burma, whilst sixteen are wanting in the Cisgangetic region. It should be remembered that a large number of the genera are widespread forms. As the result is not in agreement with the views of some who have written on the subject, the relations of species have been examined. It results that eighty-one species of mammalia belonging to the orders Primates, Carnivora, Insectivora, Rodentia, and Ungulata are recorded from the forest regions of the Himalayas. Of these 2 are doubtful, 22 are not known to occur south of the Himalayan range in India or Burma, 21 are wide ranging forms and are found in both Burma and the Indian Peninsula, 1 only (*Hystrix leucura*) is common to the Himalayan forests and the Indian Peninsula, but does not range east of the Bay of Bengal, whilst 35 are found in the countries east of the Bay of Bengal, but not in the Peninsula south of the Ganges. Of the 35, 8 only range as far as the hills south of the Assam Valley, 16 to Burma proper, and 11 to the Malay Peninsula and Archipelago. Or, in other words, of the 79 Himalayan species, 56 or 70 per cent. are common to the Transgangetic region, and only 22 or 28 per cent. to the Cisgangetic. Of the 22 species not ranging south of the Himalayas a large majority are either Holarctic species or belong to Holarctic genera.

The fauna of the Himalayan forest area is partly Holarctic, partly Indo-Malay. It is remarkably poor, when compared with the Cisgangetic and Burmese faunas, in reptiles and batrachians. It also contains but few peculiar genera of mammals and birds, and almost all the peculiar types that do occur have Holarctic affinities. The Oriental element in the fauna is very richly represented in the Eastern Himalayas and gradually diminishes to the westward, until in Kashmir and farther west it ceases to be the principal constituent. These facts are consistent with the theory that the Oriental constituent of the Himalayan fauna, or the greater portion of it, has migrated into the mountains from the eastward at a comparatively recent period. It is an important fact that this migration appears to have been from Assam and not from the Peninsula of India.

V. Southern Tenasserim agrees best in its vertebrata with the Malay Peninsula, and should be included in the Malayan sub-region of the Indo-Malay region.

The continental area of the Indo-Malay or Oriental region is divided into three sub-regions, Cisgangetic, Transgangetic, and Malayan.

There are several points left which require explanation. There is the much greater richness of the Oriental constituent in the Cisgangetic fauna to the southward in Malabar and Ceylon, although this is far away from the main Oriental area, and the occurrence also in the southern part of the Peninsula of various mammalian, reptilian, and batrachian genera, such as *Loris*, *Tragulus*, *Draco*, *Liolepis*, and *Ixalus*, which are represented in Burma and the Malay countries but not in the Himalayas or Northern India. In connection with this the limitation of the Dravidian element to the south of India should also be remembered. Then there is the occurrence of certain Himalayan species on the mountains of Southern India and Burma, and even farther south, but not in the intervening area. There is also the predominance of the Western, or what I have proposed to call the Aryan, element in the Pleistocene fauna of the Nerbudda Valley, and of Karnul in the north of the Carnatic tract. Lastly we have to account for the apparently recent immigration of Oriental types into the Himalayas.

Whilst it is quite possible that other explanations may be found, it is evident that all these peculiarities of the Indian fauna may have been due to the Glacial epoch. The great terminal moraines occurring at about 7000 feet in Sikhim, first discovered by Sir J. Hooker,* whose observations have been confirmed by myself† and others, and the occurrence of similar moraines and other indications of ice action at even lower levels in the Western Himalayas,‡ clearly show that the temperature of the mountain range must have been much lower than at the present day when no glacier in Sikhim is known to descend much below 14,000 feet.

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* ‘Himalayan Journals,’ vol. ii, pp. 7, &c.

† ‘Jour. As. Soc., Beng.,’ xl, 1871, Pt 2, p. 393.

‡ ‘Manual of the Geology of India,’ Ed. 1, p. 373; Ed. 2, p. 14, and references there quoted.

During the coldest portion of the Glacial epoch, a large part of the higher mountains must have been covered by snow and ice, and the tropical Oriental fauna which had occupied the range, and which may have resembled that of the Indian Peninsula more than is the case at present, must have been driven to the base of the mountains or exterminated. The Holarctic forms apparently survived in larger numbers. The Assam Valley and the hill ranges to the southward would afford in damp, sheltered, forest-clad valleys and hill slopes a warmer refuge for the Oriental fauna than the open plains of Northern India and the much drier hills of the country south of the Gangetic plain. The Oriental types of the Peninsula generally must have been driven southwards, and some of them, such as *Loris* and *Tragulus*, which must originally have been in touch with their Burmese representatives, have never returned. It was probably during this cold period that the ossiferous Nerbudda beds and the deposits in the Karnul caves were accumulated. The tropical damp-loving Dravidian fauna, if it inhabited Northern India, must have been driven out of the country. Unless the temperature of India and Burma generally underwent a considerable diminution, it is not easy to understand how plants and animals of temperate Himalayan types succeeded in reaching the hills of Southern India and Ceylon, as well as those of Burma and the Malay Peninsula.

When the whole country became warmer again after the cold epoch had passed away, the Transgangetic fauna appears to have poured into the Himalayas from the eastward. At the present day the comparatively narrow Brahmaputra plain in Assam is far more extensively forest-clad, especially to the eastward, than is the much broader Gangetic plain of Northern India, and if, as is probable, the same difference between the two areas existed at the close of the Glacial epoch, it is easy to see how much greater the facilities for the migration of a forest-haunting fauna must have been across the Brahmaputra Valley than over the great plain of the Ganges. This difference alone would give the Transgangetic fauna of Burma an advantage over the Cisgangetic fauna in a race for the vacant Himalayas, even if the latter had not been driven farther to the southward than the former, as it probably was during the Glacial epoch.

The theory, however, is only put forward as a possible explanation of some remarkable features in the distribution of Indian vertebrates. At the same time it does serve to account for several anomalies of which some solution is necessary. If thus accepted, it will add to the evidence, now considerable, in favour of the Glacial epoch having affected the whole world, and not having been a partial phenomenon induced by special conditions, such as local elevation.

"On the Intimate Structure of Crystals.—IV. Cubic Crystals with Octahedral Cleavage."

By W. J. SOLLAS, D.Sc., LL.D., F.R.S. Professor of Geology in the University of Oxford. Received and read March 17, 1898. Revised December 16, 1900.

Diamond. Carbon, m. w., 12; sp. gr., 3·51; m. v., 3·42. There is good reason to believe, as will appear as we proceed, that the atoms of carbon, of which diamond is composed, are disposed in the closest possible order. This mode of disposition has been described and figured by Mr. Barlow,* in his work on "A Mechanical Cause of Homogeneity of Crystals." Each atom is in contact with twelve others, and in planes normal to the trigonal axes of the cube, which they are supposed to form; they lie in closest contact, each sphere touching six others in the plane. The volume of the atoms may be most readily found by dividing the atomic volume by 1·35; it is 2·54, the diameter of the atom is 1·693, and its gross volume 4·851

The cleavage of such a closest packed assemblage as we attribute to diamond should be octahedral, for it is in planes parallel to the faces of an octahedron that the atoms lie closest together. The characteristic cleavage of diamond is thus readily accounted for; not so, however, its remarkable hemihedry. This has still to be explained. Diamond is tetrahedral, and to account for this, we may suppose its constituent atoms to be associated in groups of four, the centre of each lying at the solid angle of a tetrahedron, or we may recall the tetrahedral nature of the carbon atom, and attribute the hemihedry of diamond to an appropriate disposition of the poles of its atoms. Whichever view is adopted, it will make no difference to the subsequent treatment of the question; in either case, we have to consider the manner in which tetrahedra may be arranged so as to give rise to hemihedral symmetry. Two kinds of arrangement are possible; in both, the trigonal axes of the tetrahedra lie on the trigonal axis of a cube, but in one the tetrahedra are oppositely, in the other similarly, orientated. On bringing the former case under the notice of Mr. Barlow, he informed me that it was new to him, and subsequently he pointed out that the symmetry resulting from it is holohedral,† while the arrangement in the second case is hemihedral; we must accordingly suppose that in diamond the constituent tetrahedra, whether groups of atoms or the atoms themselves, are all similarly orientated.

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* 'Sci. Proc. Roy. Dub. Soc.,' vol. 8 (N.S.), p. 533, 1897.

† This arrangement is described by Mr. Barlow in the 'Sci. Proc. Roy. Dub. Soc.,' *loc. cit.*, under the heading C (*b*), p. 542; the alternative arrangement is given under C (*a*).

In treating of diamond, the question of dimorphism naturally presents itself; we proceed therefore to the consideration of graphite

As the recorded values for the specific gravity of graphite differ widely from one another, I made a fresh determination of this constant, using the graphite obtained as a bye-product in the manufacture of carborundum. This was kindly given me by Professor Miers. The graphite was introduced into a diffusion column of methylene iodide and benzene, it floated at a level of specific gravity 2·286, as given by small glass indicators. The molecular volume of graphite, as deduced from this, is 5·25 This value is in close accordance with that found by Petersen, who gives it as 5·3.*

The form in which graphite occurs in nature is so closely similar to a hexagonal prism, that for a long time it was referred to the rhombohedral system, but later observations show that it is decidedly oblique or monoclinic

Suppose that a number of tetrahedral groups of atoms be placed each with a trigonal axis vertical, the atoms at the base forming a single sheet in closest contact, then suppose a similar sheet placed over the first, so that the vertical trigonal axis of each of the upper tetrahedra is continuous with that of each tetrahedron of the lower layer. The resulting symmetry will be that of the oblique system, and will be hemimorphic

It is of especial interest to compare the volumetric relations which exist, on the one hand, between these hypothetical modes of packing for diamond and graphite, and on the other between the atomic volumes of diamond and graphite themselves It can be shown from mere inspection that the volume occupied by the packed spheres in the case of diamond is to that in the case of graphite as 2 : 3, for six spheres in the packing of diamond occupy the same space as four in that of graphite; *i.e.*, if we restrict attention to two sheets only, in diamond both are most closely packed, every three spheres in the lower layer having three corresponding spheres in the upper, while, in graphite, only the lower layer is most closely packed, and in the upper but one sphere occurs in correspondence to every set of three spheres below.

Next the volume found for diamond is 3·42, and that for graphite, 5·25, and 3·42 : 5·25 = 2 : 3·07. There is thus a correspondence between the ratio of the volumes as deduced from hypothesis and that obtained by experiment as exact as the nature of the case permits.

Although graphite is not truly rhombohedral, it makes a close approach to the symmetry of the rhombohedral system, as it might very well do from the structure here assigned to it. Nordenskiöld, from measurements made on graphite from Pargas, is supposed to have shown that the apparently hexagonal prisms are really oblique, produced by a combination of the forms 001, 110, 010. The angle β was found to measure 71° 16′, and thus only differs from the angle of a tetrahedron (which is 70° 30′) by 46 minutes. The angle of the prism 110 was found to be 122° 24′, or 2° 24′ greater than that of a regular hexagon.

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* 'Zeits. f Physikalische Chemie,' vol. 8, p. 601, 1891.

The packing of the atoms in graphite is the most open we have yet encountered; in diamond, not only the closest, but the closest possible. Yet graphite is the commoner form of carbon, and the more easily produced by artificial means. Nature therefore does not seem to especially favour closest possible packing, rather leaning on the contrary to the more open arrangements. The relative hardness of the two forms of carbon would seem to be connected with their structure, graphite—one of the softest of minerals, and diamond without exception the hardest. As Professor Miers suggests, the sectility of graphite may perhaps be connected with its open structure, but on other differences in the physical characters of the two substances the structure seems to throw no light.

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